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Some Arguments in Defense of the Venetians on the Fourth Crusade

DONALD E. QUELLER
and
GERALD W. DAY

OF THE MANY MYTHS clouding Venetian history none has been more influential and persistent than the pejorative one concerning the city's role in the Fourth Crusade. The indictment of Venice was stated most eloquently by John Ruskin, who wrote that "the expedition which was commanded by the noblest of her princes, and whose results added most to her military glory, was one in which while all Europe around her was wasted by the fire of its devotion, she first calculated the highest price she could exact from its piety for the armament she furnished, and then, for the advancement of her own private interests, at once broke her faith and betrayed her religion."¹ Since Louis de Mas Latrie in the mid-nineteenth century first enunciated the classic scholarly theory of Venetian treason in the Fourth Crusade many historians have charged that Dandolo and the Venetians schemed to exploit the religious zeal of the crusaders to gain their own commercial ends.²

Donald M. Nicol in the new *Cambridge Medieval History* accurately sums up the prevailing view with which he ardently agrees: "In the interminable controversy over the diversion of the Fourth Crusade, many have doubted the ulterior motives of Philip of Swabia, of Boniface of Montferrat, and of Innocent III. But few have tried to exonerate the Venetians."³ Even scholars who are otherwise well disposed toward Venice, such as the distinguished Venetophile, Frederic C. Lane, often allow the myth of Venetian duplicity to intrude into their writings,⁴ and Venice's staunchest defender, Roberto Cessi, obviously strains to offer plausible vindication of her role in the great misadventure.⁵

¹ John Ruskin, *The Stones of Venice* (New York, 1884), 1: 6.

² Louis de Mas Latrie, *Histoire de l'île de Chypre sous le règne des princes de la maison de Lusignan* (Paris, 1852-1861), 1: 161-4. For a survey of the treason theorists to 1969, see Donald E. Queller and Susan J. Stratton, "A Century of Controversy on the Fourth Crusade," *Studies in Medieval and Renaissance History*, 6 (1969), 238-52.

³ Donald M. Nicol, "The Fourth Crusade and the Greek and Latin Empire, 1201-1261," *Cambridge Medieval History* (Cambridge, 1966), 4: 278.

⁴ Frederic C. Lane, *Venice: A Maritime Republic* (Baltimore, 1973), 45.

⁵ Roberto Cessi, "L'eredità di Enrico Dandolo," *Archivio veneto*, ser. 5, 67 (1960), 2, n. 2; "Venezia e la quarta crociata," *Archivio veneto*, ser. 5, 48-69 (1951), 11, n. 1; 26-67. His views have not been widely accepted.

Among the anti-Venetian assumptions of most historians of the Fourth Crusade, the following are prominent:

1. A religious crusading enthusiasm was foreign to the Venetian psyche.⁶
2. Venice compelled the crusading host to comply with its treacherous plans.⁷
3. The Egyptian target upon which the Venetians and the crusaders' envoys had agreed in early 1201 was disadvantageous to Venice's mercantile interests.⁸
4. Venice was suffering setbacks in Constantinople that focused her attention exclusively upon securing a stable monopolistic position in the Greek Empire.⁹

We are not trying to make the Venetians the heroes of the crusade, while transferring the charge of treason to the Germans, the pope, or some other candidate. We do intend to marshal evidence to prove that these four widespread assumptions are too shaky to sustain the weight of the anti-Venetian theories that scholars have built upon them. They do seem plausible, even self-evident, in the light of the eventual profit that Venice derived from the conquest of Constantinople; a close scrutiny of the evidence concerning the crusade and the overall Levantine economic situation at the beginning of the thirteenth century, however, will raise doubts about the culpability of the Venetians. We believe that they were the victims, and in the end the beneficiaries, like the other crusaders, of a series of mistakes, failures, and accidents that determined the course of the Fourth Crusade. The Venetians were caught up in a chain of unforeseen events that threatened to overwhelm the crusade, and it was they and their inimitable Doge Dandolo who consistently provided the leadership to rescue the crusading host.

There is a widely held misconception among historians of the Fourth Crusade that the Venetian interest in the crusading movement was radically different from that of the northern knights and morally and religiously inferior to it. While the northern crusader is usually portrayed as driven by sublime motives of self-sacrifice and even martyrdom, the Venetian is pictured as a greedy opportunist whose sole consideration was profit. In fact, the differences in background and viewpoint between the two groups are minor in contrast to the values they shared as members of Latin Christendom.

⁶ For example: Freddy Thiriet, "Le quart et demi de la Romanie," in Jacques Goimard, ed., *Venise au temps des galères* (Paris, 1968), 72; Gina Fasoli, "Nascita di un mito," in *Studi storici in onore di Gioacchino Volpe* (Florence, 1958), 462; Edgar H. McNeal and Robert Lee Wolff, "The Fourth Crusade," in Kenneth Setton, ed., *A History of the Crusades*, 2d ed. (Madison, 1969), 2: 161.

⁷ For example: Charles Diehl, "The Fourth Crusade and the Latin Empire," *Cambridge Medieval History* (Cambridge, 1936), 4: 416; Steven Runciman, *A History of the Crusades* (Cambridge, 1953-57), 3: 114.

⁸ For example: Mas Latrie, *Histoire de l'île de Chypre*, 1: 163-4. William Hazlitt, *The Venetian Republic: Its Rise, its Growth, and its Fall, A.D. 409-1797* (London, 1915), 1: 266-7.

⁹ The *locus classicus* for this assumption is Nicetas Choniates, *Historia*, ed. by Immanuel Bekker (Bonn, 1835), 713-4. It is also one of the main themes of Walter Norden's admirable work, *Der vierte Kreuzzug im Rahmen der Beziehungen des Abendlandes zu Byzanz* (Berlin, 1898). See also McNeal and Wolff, "Fourth Crusade," 161-2; George Ostrogorsky, *History of the Byzantine State*, trans. by Joan Hussey (Oxford, 1956), 368; A. A. Vasiliev, *History of the Byzantine Empire, 324-1453* (Madison, 1952), 452-3; Diehl, "Fourth Crusade," 417; Steven Runciman, "Byzantine Trade and Industry," *Cambridge Economic History* (Cambridge, 1941-1965), 2: 101.

We must, indeed, accept only with reservations the exaggerated religiosity of the knightly crusader. Although true devotion undoubtedly set many men on their warlike pilgrimage, the hope of earthly gain was certainly a significant motive for crusading leaders such as Baldwin of Boulogne, who carved out a personal lordship around Edessa, Bohemund of Taranto, who became prince of Antioch, and Raymond of Saint-Gilles, who spent the last years of his life in an unsuccessful attempt to make Tripoli his own. It is well known that at the very conception of the First Crusade Urban II aroused the cupidity of his audience at Clermont by urging the conquest of "the land of milk and honey."¹⁰

The crusaders' zeal for liberating relics also had its greedy aspect. The Fourth Crusade is especially notorious for the vast number of these venerable objects that were ecstatically carted off from Constantinople to the West.¹¹ In this light it seems somewhat ironical that Gunther of Pairis, who berated the Venetians for their greed, glorified his abbot's successful looting of Constantinople's hoard of precious relics.¹²

When compared to the worldly-wise creators of the crusading states and to the zealous souls who plundered relics, the Venetians cannot be regarded as unique. The factor distinguishing the knight, the ecclesiastic, and the merchant from one another in this respect was only what they considered to be of value. The feudal baron desired territorial dominion, the priest or monk a trove of sacred souvenirs, and the Italian merchant commercial advantage. The person brought up in a feudal or ecclesiastical milieu might well fail to understand and sympathize with the values of the parvenu bourgeois. Yet on the whole these values are not inherently inferior to feudal values or even to some of those for which the church had stood: in contexts other than the crusades historians of the High Middle Ages generally regard favorably the emergence of bourgeois standards. In fact, the negative attitude of the feudality toward the Venetians probably has been exaggerated. A thorough reading of Villehardouin and Robert of Clari will turn up no instance of anti-Venetian feeling on the part of either author, although both of them were thoroughly imbued with the values of feudal society. Both chroniclers, in fact, are full of praise for their Venetian allies.

We do not deny, of course, that the Venetians could be strongly moved by commercial interests, but there is also evidence of their genuine religious enthusiasm for the Fourth Crusade. Where differing motives point toward the same goal it is an audacious and simplistic historian who insists upon one to the exclusion of others. Villehardouin, in fact, presents the Venetians as a

¹⁰ This phrase, taken from Exodus, 3:8, occurs in Robert the Monk's version of Urban's sermon in a context contrasting the rewards of Palestine with the future crusaders' poverty at home. *Recueil des historiens des croisades, historiens occidentaux* (Paris, 1844-95), 3: 728-9. For a brief discussion of the promise of worldly rewards mentioned in the various versions of Urban's sermon, see Dana C. Munro, "The Speech of Urban II at Clermont," *American Historical Review*, 11 (1906), 239.

¹¹ Paul Riant compiled three volumes of materials on such translations, *Exuviae sacrae Constantinopolitanae* (Geneva and Paris, 1877-1904).

¹² Gunther of Pairis, *Historia Constantinopolitana*, in Riant, ed., *Exuviae*, 1: 71, 85, 104-8, 121-2, 125-6.

people intensely moved by the religious aspects of the crusade. His hyperbolic account of their reception of the proposal to join the crusade is marked by their pious fervor:¹³

There the six envoys knelt at the feet of the people, weeping many tears. And the Doge and all the others burst into tears of pity and compassion, and cried with one voice, and lifted up their hands, saying: "We consent!" Then there was so great a noise and tumult that it seemed as if the earth itself were falling to pieces. And when this great tumult and passion of pity—greater did never any man see—were appeased, the good Doge of Venice, who was very wise and valiant, went up into the reading desk and spoke to the people and said: "Signors, behold the honour that God has done for you; for the best people in the world have set aside all other people, and chosen you to join them in so high an enterprise as the deliverance of our Lord."

Later, when the expedition had been prepared and was on the point of departure, the doge himself and many of his subjects took the crusading vow in Saint Mark's in an emotion-packed display of zeal for the pious mission:¹⁴

Before the beginning of High Mass, the Doge of Venice, who bore the name of Henry Dandolo, went up into the reading desk, and spoke to the people, . . . "Signors, you are associated with the most worthy people in the world, and for the highest enterprise ever undertaken; and I am a man old and feeble, who should have need of rest, and I am sick in body; but I see that no one could command and lead you like myself, who am your lord. If you will consent that I take the sign of the cross to guard and direct you, and that my son remain in my place to guard the land, then shall I go to live or die with you and with the pilgrims." And when they had heard him, they cried with one voice: "We pray you by God that you consent, and do it, and that you come with us!" Very great was then the pity and compassion on the part of the people of the land and of the pilgrims; and many were the tears shed, because that worthy and good man would have had so much reason to remain behind, for he was an old man, and albeit his eyes were unclouded, yet he saw naught, having lost his sight through a wound in his head. . . . Thus he came down from the reading desk, and went before the altar, and knelt upon his knees greatly weeping. And they sewed the cross onto a great cotton hat, which he wore, because he wished that all men should see it. And the Venetians began to take the cross in great numbers.

By this act the Italian merchants transformed themselves from purely secular dealers in goods and services to *milites Christi*, enjoying the religious perquisites and accepting the obligations of crusaders. J. Lestocquoy has made the point, which needs to be emphasized, that among the medieval bourgeois, "*la foi chrétienne n'était pas un décor.*"¹⁵

In response to the charge that Venice participated minimally in the crusades and only for her own profit, it can be shown that the Republic had invested considerable resources in the Holy War. It is true that Venice did not take part in the initial operations of the First Crusade, but this hesitancy was

¹³ Geoffroi de Villehardouin, *La conquête de Constantinople*, ed. by Edmond Faral (Paris, 1938-9), 1: 28-30. Translation by Sir Frank T. Marzials, *Memoirs of the Crusades* (New York, 1958), 8.

¹⁴ Villehardouin, *Conquête de Constantinople*, 1: 66-70. See especially 1: 68-70 for Venetian emotions. Trans. by Marzials, *Memoirs of the Crusades*, 16-7.

¹⁵ J. Lestocquoy, *Aux origines de la bourgeoisie: les villes de Flandre et d'Italie sous le gouvernement des patriciens (XI^e-XV^e)* (Paris, 1952), 125.

quite possibly not caused by religious apathy, but by extremely strong practical considerations that outweighed Venetian piety. Norman influence was strong in the First Crusade.¹⁶ The Venetians may well have feared, as did their Byzantine patrons, that the expedition would turn out to be a re-enactment of Robert Guiscard's abortive invasion of the Byzantine Empire only twenty years before. If such an eventuality had taken place with Venetian help, the city's vital friendly relations with Constantinople would have been seriously jeopardized. Any addition to Norman strength, moreover, would have increased the threat of Norman control of the Adriatic and the loss of unrestricted Venetian access to the Mediterranean.¹⁷ Once the Norman influence over the crusade had been diminished when Bohemund withdrew from the expedition to rule his new principality of Antioch, the Venetians in 1099 sent a large fleet under the doge himself that reprovisioned the destitute Christian army lingering at newly captured Jaffa.¹⁸ In return for commercial concessions in crusader territories, the Venetian force supported Godfrey of Bouillon's army with supplies, war engines, and men during the crusaders' successful sieges of the castle of Acheron and Haifa.¹⁹ A Venetian fleet of almost a hundred ships also helped Baldwin I conquer Sidon in 1110.²⁰

Venice's great contribution to the crusader states came in the early 1120s when a Venetian armada crushed the Fatimid navy at the battle of Ascalon and gave decisive support to the capture of Tyre.²¹ As the situation in Syria settled down to an uneasy coexistence between the crusaders and their Muslim adversaries, the need for further Venetian participation in the crusading movement vanished for a time. During the Second Crusade, the naval support that the Venetians were giving to the Byzantine Empire against Roger II of Sicily prevented the city from taking part in the Palestinian enterprise.²²

Venice dutifully came to the aid of Christendom again in the Third Crusade. The city quickly responded to the Muslim challenge by sending a large fleet to the Holy Land in 1188, although nothing is heard of the fleet in action.²³ In the next year, however, a joint Pisan and Venetian fleet sailed east and gave significant help to the small Christian army besieging Acre.²⁴ In order to

¹⁶ Sidney Painter, "Western Europe on the Eve of the Crusades," in Setton, ed., *History of the Crusades*, 1: 21.

¹⁷ Gino Luzzatto, *An Economic History of Italy: From the Fall of the Roman Empire to the Beginning of the Sixteenth Century*, trans. by Philip Jones (London, 1961), 73; James Westfall Thompson, *An Economic and Social History of the Middle Ages* (New York, 1928), 330-3.

¹⁸ The Venetians gave some supplies to the crusaders and sold them others, *Translatio Sancti Nicolai ad Venetiam*, in *Recueil des historiens des croisades, historiens occidentaux*, 5: 271. See also *Annales venetici breves*, in *M.G.H., SS.*, 14: 70, and Andrea Dandolo, *Chronica*, ed. by E. Pastorello, in *R.I.S.*, 12: 222-3. For a differing interpretation, see Freddy Thiriet, *La Romanie vénitienne au moyen âge* (Paris, 1959), 40.

¹⁹ *Translatio Sancti Nicolai*, 272, 275-7.

²⁰ Dandolo, *Chronica*, 264.

²¹ Runciman, *History of the Crusades*, 2: 166-7; Robert L. Nicholson, "The Growth of the Latin States, 1118-1144," in Setton, ed., *History of the Crusades*, 1: 421.

²² Virginia G. Berry, "The Second Crusade," in Setton, ed., *History of the Crusades*, 1: 486; Dandolo, *Chronica*, 243; G. L. Tafel and G. M. Thomas, eds., *Urkunden zur älteren Handels- und Staatsgeschichte der Republik Venedig* (Vienna, 1856-7), 1: 113-24.

²³ *Annales venetici breves*, 72.

²⁴ Dandolo, *Chronica*, 270.

concentrate the Venetian effort on aid to the Holy Land, the doge in November 1188 ordered all Venetians from various parts of the world to return to Venice by Easter.²⁵ Here is an example of the Venetians specifically and explicitly sacrificing their commercial activities in the interest of the crusade.

In the course of the Third Crusade, moreover, Venice remained aloof from the intense crusader factionalism between the Montferrine and Lusignan parties, while the Pisans and the Genoese, on the contrary, sought to enlarge their Syrian establishments by playing one side off against the other.²⁶ It is probably in part for this reason that the Venetians are infrequently mentioned in the non-Venetian sources for the crusade, which deal at length with this struggle.²⁷ All that Venice received for its services, and apparently all that it asked, was a confirmation of its pre-Saladinic privileges and properties outlined in the treaty of 1123 with Patriarch Warmund of Jerusalem.²⁸ In short, while there is little doubt that a major reason for Venetian participation in the Third Crusade was to regain what the republic had lost by Saladin's invasion, Venice alone of the major Italian mercantile cities did not exploit the plight of the Christian Holy Land and the ruinous strife among the crusaders themselves to enhance its existing commercial privileges.

As a final entry, prior to 1201, into this respectable record of aid to the Holy Land, Venice, once again unlike the other mercantile cities, responded favorably in 1198 to a papal legate sent to gain support for the new crusade.²⁹ This continuing accessibility to crusading overtures must have influenced the decision of the ambassadors who in 1201 came to Italy to find transportation for the new crusade that was being marshaled in the heart of France.³⁰

Envoys of the counts of Flanders, Champagne, and Blois concluded a contract for transportation of the crusaders with the Venetians in March or April of 1201.³¹ The agreement called for the passage of 33,500 Christian soldiers and 4,500 horses and their maintenance for one year at a total price of 85,000 Cologne marks. The Venetians also agreed to join the crusade them-

²⁵ *Ibid.* The document itself is published in Tafel and Thomas, eds., *Urkunden*, 1: 204-6.

²⁶ *Infra*, p. 733.

²⁷ Richard of London, for example, often mentions the Pisans and the Genoese, but never the Venetians. *Itinerarium peregrinorum et gesta regis Ricardi: auctore ut videtur Ricardo canonico S. Trin. Lond.*, ed. by William Stubbs (London, 1864), 61, 109, 136, 137, 174, 212, 227, 228, 321-3, 405, 413, 416 (for the Pisans); 84, 151, 174, 206, 321-3, 405, 413, 414, 416 (for the Genoese; Venice is mentioned only once, and then only as the destination of a part of the crusading force on its way east (152).

²⁸ Tafel and Thomas, eds., *Urkunden*, 1: 84-9.

²⁹ *Gesta Innocentii Papae III*, Migne, PL, CCXIV, xc.

³⁰ On this embassy, see D. E. Queller, "L'évolution du rôle de l'ambassadeur: les pleins pouvoirs et le traité de 1201 entre les croisés et les vénitiens," *Le Moyen Age*, 67 (1961), 479-501. Of the three major Italian commercial cities probably only Venice had the capability to transport the huge army of 33,500 men that was expected to assemble. Even so, Venice, the greatest of the maritime powers, had to strain all of its resources to meet its contractual obligations. *Infra*, pp. 724-6. The likelihood of Genoese or Pisan participation was slight because of their war against each other. *Gesta Innocentii*, xci.

³¹ The date of the treaty has not been satisfactorily solved. Villehardouin, *Conquête de Constantinople*, 1: 30-2, says that the treaty was concluded during Lent, that is, prior to March 25, but the extant treaty itself bears a date of April 1201. For attempted reconciliations, see C. Klimke, *Die Quellen zur Geschichte des vierten Kreuzzugs* (Breslau, 1877), 83, n. 2; Faral, in Villehardouin, *Conquête de Constantinople*, 1: 219; Jean Longnon, *Recherches sur la vie de Geoffroy de Villehardouin, suivies du catalogue des actes des Villehardouin* (Paris, 1939), 69; Queller, "L'évolution du rôle de l'ambassadeur," 494. *Contra*, see Colin Morris, "Geoffrey de Villehardouin and the Conquest of Constantinople," *History*, 53 (1968), 30-1.

selves with fifty galleys, and it was further provided that they should receive one-half of all conquests.³²

The tragedy of the Fourth Crusade is found in the gross overestimation of the size and resources of the crusading army. The projected force would have been three times the size of the one mustered by Philip Augustus for his most crucial battle, Bouvines, in 1214, and the price was double the annual revenues of the king of England or the king of France.³³ It is true, of course, that the count of Flanders, who ruled a highly industrialized area, and the count of Champagne, in whose domain the profitable fairs took place, were two of the wealthiest French nobles, but events were to prove the incapacity of the crusaders to muster such a large force.

The Venetians have been accused of trapping the gullible crusaders in a contract that they knew the northerners could not fulfill so they might have the army at their mercy, but this argument will not stand.³⁴ Even though Villehardouin puts into the mouth of Doge Dandolo the first specific proposal of terms for the contract, it is inconceivable, as Colin Morris says, that the envoys did not provide the estimate of the number of crusaders to be accommodated.³⁵ The envoys of the three counts, moreover, were not such dolts as Hellwig makes them out to be. Geoffrey of Villehardouin, the spokesman, was a mature man of about fifty, marshal of the prosperous county of Champagne. He enjoyed then and continued to enjoy an excellent reputation for his political *savoir faire*.³⁶ It is unlikely, moreover, that envoys from the most economically advanced parts of northern Europe were ignorant of financial negotiations and contracts. Hellwig's view rests upon an obsolete concept of economic life at the beginning of the thirteenth century.

If the crusaders, as would normally be expected, stated the number of men for whom transportation was desired, the Venetians presumably suggested the price, which, it has been charged, was extortionate. Fortunately there are two extant transportation contracts for crusaders from the late twelfth century that shed some light on the cost of the contract of 1201. In 1184 Genoa agreed to carry thirteen knights, twenty-six horses, and twenty-six squires provisioned for eight months at a cost of eight and a half marks for each theoretical unit of one knight, two horses, and two squires.³⁷ In 1190 Philip Augustus contracted with Genoa to ship his crusading army of 650 knights, 1,300 horses, and 1,300 squires and to maintain them for eight months for a total price of

³² Tafel and Thomas, eds., *Urkunden*, 1: 365-7.

³³ Lane, *Venice*, 37.

³⁴ Martin Hellwig, "Die ritterliche Welt in der französischen Geschichtsschreibung des vierten Kreuzzuges," *Romanische Forschungen*, 52 (1938), 1-40, provides the clearest statement of the position.

³⁵ Villehardouin, *Conquête de Constantinople*, 1: 22-4; Morris, "Geoffrey de Villehardouin," 30-1.

³⁶ Faral, in Villehardouin, *Conquête de Constantinople*, 1: vii. During the crusade and after we find him charged with many diplomatic and military responsibilities. For a list of his missions, see Queller, "L'évolution du rôle de l'ambassadeur," 482-3, n. 11, although no. 2 should be properly attributed to Geoffroy de Joinville, rather than to Villehardouin.

³⁷ Auguste Jal, *Memoire sur quelques documents Genoises relatifs aux deux croisades de Saint Louis* (Paris, 1842), 41. The numerical comparisons were made, although outside a diversion context, by Ronald P. Grossman, "The Financing of the Crusades," (Ph.D. dissertation, University of Chicago, 1965), 17-8.

5,580 marks.³⁸ In this instance, each theoretical unit of the same sort cost Philip nine marks. Extending these eight-month contracts to a year to make them comparable to the treaty of 1201 would have raised the price of the contract of 1184 to twelve and three-quarters marks per unit and that of Philip Augustus to thirteen and a half marks.

The contract of 1201 stipulated transportation and a year's provisions for 4,500 knights, 4,500 horses, 9,000 squires, and 20,000 foot soldiers for 85,000 marks. This price figures out to two marks per man and four marks per horse.³⁹ Under these terms one knight, two horses, and two squires would cost fourteen marks. Hence, the Venetian price was only half a mark per unit higher than the Genoese charge of ten years earlier. The French, moreover, had not been satisfied with Genoese performance,⁴⁰ while our sources have only praise for the fleet prepared by Venice.⁴¹ Though higher than the Genoese price, the Venetian charge does not seem extortionate; it may only have taken into account the additional cost required to assure satisfactory service.

The preparation of a fleet of this magnitude represented a massive effort on the part of the Venetians. We may estimate the size of the fleet needed and, in fact, ready and waiting for the crusaders in June 1202 as about five hundred vessels, not counting petty auxiliary craft.⁴² To appreciate the magnitude of this undertaking, it is instructive to contrast it with the vastly inferior capabilities of the famed Venetian Arsenal in the fourteenth and fifteenth centuries.⁴³ Although the Venetian peacetime fleet was large, many additional transports and galleys had to be built.⁴⁴ The horse transports probably ran up the cost, moreover, because of the size of the animals and the special outfitting required to load and unload them and to keep them from being pitched about with the rolling of the ships.⁴⁵ In addition, many of these vessels, especially the

³⁸ Cesare Imperiale di Sant' Angelo, ed., *Codice diplomatico della repubblica di Genova* (Rome, 1936-42), 2: 364-8.

³⁹ A higher price was assigned to the horses because of their size and the special accommodations required for them. *Infra*, pp. 724-5.

⁴⁰ H. Vriens, "De kwestie van den vierden kruistocht," *Tijdschrift voor Geschiedenis*, 37 (1922), 61.

⁴¹ "Et li navies que il orent appareille fu si riches et si bels que onques nus hon crestiens plus bel ne plus riche ne vit; si cum de nes et de galies et de uissiers bien a troiz tanz que it n'aust en l'ost de genz." Villehardouin, *Conquête de Constantinople*, 1: 58. "Quant le pelerin furent tot asanle en Venice et il virent le rike navie que faite estoit, les rikes nes, les grans dromons et les uissiers a mener les chavax et les galies, si s'en merveillierent molt et de le grant rikeche que il troverent en le vile." Robert of Clari, *La conquête de Constantinople*, ed. by Philippe Lauer (Paris, 1924), 9.

⁴² The fleet actually used numbered about 200 ships. The *Devastatio Constantinopolitana*, ed. by Charles Hopf in *Chroniques Gréco-Romanes* (Berlin, 1873), 87 (also in *M.G.H.*, SS, 16: 10); and Hugh of St. Pol, *Epistola*, in Tafel and Thomas, eds., *Urkunden*, 1: 306, are very near to that number. Nicetas Choniates, *Historia*, 714, estimated slightly over 240. Villehardouin informs us that there were ships ready for three times as many crusaders as actually appeared in Venice. *Conquête de Constantinople*, 1: 58. The fifty Venetian galleys were not for transport, of course, so there were about 150 transports in the actual fleet, and, if Villehardouin is correct, some 450 transports prepared for men and horses—plus the 50 galleys.

⁴³ Frederic C. Lane, *Venetian Ships and Shipbuilders of the Renaissance* (Baltimore, 1934), 132-5. I have corresponded with Lane concerning my calculations, and I believe that he finds them reasonable, although he cautions that Villehardouin's "three times" may be a landlubber's guess. Villehardouin, unlike many medieval writers, however, customarily uses numbers carefully and accurately.

⁴⁴ Before leaving Venice in the spring of 1201 the crusaders' envoys borrowed money from a Venetian bank to enable Venice to begin construction of the fleet. Villehardouin, *Conquête de Constantinople*, 1: 32.

⁴⁵ Michel Mollat, "Problèmes navals de l'histoire des croisades," *Cahiers de civilisation médiévale*, 10 (1967), 352-3.

horse transports, were not likely to be of subsequent use. It might be years before an expedition of comparable size would sail by way of Venice, and in the meantime these wooden ships would either rot away or require extensive repair. Lane estimates the average life expectancy of a Venetian vessel at ten years.⁴⁶ Very few, if any, of the Fourth Crusade ships, therefore, would have been usable even for the Fifth Crusade. The rapid decay of ships precluded also the use for the Fourth Crusade of those ships that had been built for the Third Crusade.

Robert of Clari gives two glimpses of the sacrifices that the Venetians made to fulfill their contractual obligations. He relates that Venice had to conscript one-half its manpower to arm and man the galleys and the transports.⁴⁷ If we accept Carile's estimate that a fleet of this size required a complement of 17,264 men,⁴⁸ and we consider that the population of Venice, young men and old, women and children, was probably less than 100,000,⁴⁹ Robert's account gains credence.

Robert also mentions twice an interdiction of a year and a half that the Venetians imposed upon their trade to prepare for the crusade.⁵⁰ Although Robert is unreliable for events before his own arrival in Venice, the second mention of the interdiction comes after that time when the doge was complaining to the crusaders about their inability to pay their debt. This prohibition represents a considerable sacrifice for a city that lived by its trade, but such a suspension, as we have seen, was not an unheard-of measure. In 1162 and 1163 Genoa called off all trade with the Levant to prepare for an attack in alliance with Barbarossa against Norman Sicily, although the emperor later backed out of the undertaking.⁵¹ More to our point, in 1188-89 the Venetians cancelled trading ventures in preparation for the Third Crusade.⁵² Later, in 1215, the Fourth Lateran Council attempted to call off Levantine commerce for four years to ensure sufficient shipping for the Fifth Crusade.⁵³

The Venetian state was accustomed to regulating its merchant fleets.⁵⁴

⁴⁶ Lane, *Venetian Ships and Shipbuilders*, 263.

⁴⁷ *Conquête de Constantinople*, 9.

⁴⁸ Antonio Carile, "Alle origini dell'impero de'Oriente: analisi quantitativa dell'esercito crociato e ripartizione dei feudi," *Nuova rivista storica*, 56 (1972), 287-8. We think it may be high, but, in any case, the burden was very heavy.

⁴⁹ Lane, *Venice*, 36. Thiriet estimates the Venetian population at 50,000. *Romanie vénitienne*, 66, n. 1. In all fairness we must add that Venice also drew manpower from its Adriatic possessions, although we have no way of knowing the numbers. C. Manfroni, *Storia della marina italiana dalle invasioni barbariche alla caduta di Costantinopoli* (Livorno, 1899), 1: 317-8.

⁵⁰ *Conquête de Constantinople*, 8 and 10.

⁵¹ Yves Renouard, *Les hommes d'affaires italiens au moyen âge* (Paris, 1968), 68-9.

⁵² Dandolo, *Chronica*, 270. For the actual order requiring all Venetian merchants to return to Venice by Easter 1189, see Tafel and Thomas, eds., *Urkunden*, 1: 204-6.

⁵³ Hans Eberhard Mayer, *The Crusades*, (Oxford, 1972), 208.

⁵⁴ Lane, *Venice*, 49-50. The Venetian contracts concerning the ship *Paradiso*, sailing to Syria in the summer of 1201, however, indicate that the prohibition was not absolutely enforced. Raimondo Morozzo della Rocca and Antonio Lombardo, eds., *Documenti del commercio veneziano nei secoli XI-XIII* (Turin, 1940), 1: 451-2; Raimondo Morozzo della Rocca and Antonio Lombardo, eds., *Nuovi documenti del commercio veneto dei secoli XI-XIII*, in *Monumenti Storici della Deputazione di Storia Patria* (Venice, 1953), 7: 59-60. There are several possible explanations for this discrepancy. The possibility that Robert may have been wrong is conceded, but just as likely, the *Paradiso* could have sailed unlawfully, or especially, considering the huge size of this ship, it may have been allowed to sail if preparations for its departure had been in progress when the doge announced his ban. It is worth noting in regard to the last possibility that the *Paradiso* was one of the great

What the cessation probably amounted to was the cancellation of one or both voyages of 1201. If the spring convoy, which usually set out sometime between Easter and the end of May, had not yet departed in 1201 before the conclusion of the treaty, then it was cancelled. The summer convoy, which should have departed between June 24 and the end of September, was surely kept home.⁵⁵ Voyages apart from the convoys, of course, were also cancelled. Since the sailing date fixed by the treaty was June 29, 1202,⁵⁶ although the fleet did not actually set out until the beginning of October,⁵⁷ the crusade itself took the place of the spring sailing, and perhaps also the summer convoy of that year. The loss of revenue from the cancelled voyages would have been a severe drain upon the Venetian economy if it could not be made up in some way.

The Venetian claim to half the conquests to be made by the crusade was hardly excessive. This reward was in return for the fifty galleys and Venetian participation as crusaders, above and beyond the provision of transportation.⁵⁸ The Venetians drafted half their adult males to go on the expedition.⁵⁹ When the crusade finally got under way there were probably approximately three Venetians for every two crusaders.⁶⁰ In the fighting before Constantinople in 1203 and 1204 the Venetians acted as the equal allies of the northern crusaders, as they were, according to the terms of the treaty.⁶¹

Since Venice had sacrificed so much in its expenditure of labor, material, money, expectation of profits, and men, one can easily understand Dandolo's anger when the host could not pay the agreed carrying fee. The doge spoke to the crusaders assembled in Venice, stressing his city's prodigious commitment, and he threatened to cut off their supplies unless they paid up. Rather than being part of an overall plan to force the crusaders to submit to Venetian direction, Dandolo's threat represents simply the angry reaction of a man who saw his city's efforts and investment perishing for nothing. Like the honorable and wise man that he was, however, when his temper abated he forgot his vain threat, and he began searching for a more realistic solution.⁶²

A solution, indeed, had to be found quickly. The crusaders were gathered on the island of San Nicolò di Lido in a sort of limbo brought on by their penury. They could neither pay to sail nor afford to stay. From the high prices commanded for food, one can conclude not only that individual Venetians were profiteering,⁶³ which they probably were, but that Venice was having a

transports on the crusade and it played a hero's role. Villehardouin, *Conquête de Constantinople*, 2:44. We owe the reference to the *Paradiso* and to the commercial researches to Louise Buenger Robbert.

⁵⁵ For the customary dates, Wilhelm Heyd, *Histoire du commerce du Levant au moyen âge* (Amsterdam, 1959), 1: 180-1.

⁵⁶ Tafel and Thomas, eds., *Urkunden*, 1: 366.

⁵⁷ Villehardouin, *Conquête de Constantinople*, 1: 76; *Devastatio*, 87 (*M.G.H.*, SS, 16:10); *Gesta episcoporum Halberstadensium*, ed. by L. Weiland, *M.G.H.*, SS., 23: 117. There is a discrepancy of as much as a week in the date.

⁵⁸ Villehardouin, *Conquête de Constantinople*, 1: 24.

⁵⁹ Robert of Clari, *Conquête de Constantinople*, 9.

⁶⁰ *Supra*, p. 725, for 17,264 Venetians. *Supra*, n. 41 for one-third of the anticipated 33,500 crusaders.

⁶¹ For example: Villehardouin, *Conquête de Constantinople*, 1: 162, 174-8; 2: 10, 34, 44.

⁶² Robert of Clari, *Conquête de Constantinople*, 10.

⁶³ *Devastatio*, 87 (*M.G.H.*, SS., 16:10). Poor harvests in recent years also undoubtedly affected the price of food. Robert of Auxerre, *Chronicon*, ed. by O. Holder-Egger, *M.G.H.*, SS., 26: 261.

difficult time supplying the army without breaking into the year's provisions intended for the journey and the campaign.⁶⁴ Though the crusaders seemed to be imprisoned on the island,⁶⁵ the Venetians were not responsible. The crusaders were locked in by their own inability to pay. Robert of Clari stated, moreover, that it was they themselves who had chosen to camp on the Lido,⁶⁶ although the anti-Venetian *Devastatio* complains that they were deliberately isolated by their hosts.⁶⁷ Whoever made the choice, it was a reasonable one. Venice proper certainly could not house an anticipated army more than one-third the size of the population of the city, and that number of soldiers with nothing but time on their hands would have constituted a considerable threat to the peace of the citizens.

The encampment on the Lido would have been suitable for the short sojourn that had been anticipated, but the crusaders' stay was protracted for months, while the early arrivals awaited the latecomers. The scheduled date of departure, June 29, passed. The papal legate did not arrive until July 22.⁶⁸ Near the end of July Abbot Martin of Pairis also arrived at the head of 1200 Germans.⁶⁹ The bishop of Halberstadt came on August 13.⁷⁰ The chief of the crusade, Boniface of Montferrat, did not reach Venice until August 15.⁷¹ Within three weeks of the arrival of the marquis of Montferrat a solution had been found, a make-shift plan to extricate the army from the Lido while regaining Zara for Venice.⁷²

Critics of the Venetians' role in the Fourth Crusade seem to feel that they should simply have absorbed their losses and sailed off on a wave of crusading enthusiasm, a suggestion that is neither realistic nor just. It is true, of course, that Venice had a strong interest in the reconquest of Zara and that the assistance of the crusaders against the city was made a *quid pro quo* for putting the crusade to sea with 34,000 marks of the debt to Venice outstanding.⁷³ Zara was not intended, however, as a substitution for the crusading goal, but as an excursion on the way. Such attacks against Christian enemies were common for Italian crusading fleets. The Pisan armada of 1099 under Archbishop Daimbert on its way to the Holy Land raided the islands off the Heptanese—Corfu, Leucas, Cephalonia, Zante, and Cyprus.⁷⁴ The Venetian fleet of

⁶⁴ The problem of feeding the crusaders before the embarkation may account for the discrepancy between the provision of the treaty of 1201 that the Venetians would supply food for a year after embarkation (Tafel and Thomas, eds., *Urkunden*, 1: 365) and Villehardouin's statement that they would be provided for nine months (*Conquête de Constantinople*, 1:22).

⁶⁵ *Devastatio*, 87 (*M.G.H.*, SS., 16:10); *Gesta episcoporum Halberstadensium*, 118.

⁶⁶ *Conquête de Constantinople*, 9.

⁶⁷ *Devastatio*, 87 (*M.G.H.*, SS., 16:10).

⁶⁸ *Ibid.*

⁶⁹ Gunther of Pairis, *Historia Constantinopolitana*, 67 and 70. Francis Swietek has convinced us that this figure should be used with caution.

⁷⁰ *Gesta episcoporum Halberstadensium*, 116.

⁷¹ *Devastatio*, 87 (*M.G.H.*, SS., 16:10).

⁷² Villehardouin, *Conquête de Constantinople*, 1:66; Robert of Clari, *Conquête de Constantinople*, 11–2.

⁷³ Villehardouin, *Conquête de Constantinople*, 1: 64. Robert of Clari, who is extremely unreliable on numbers, says 36,000 marks. *Conquête de Constantinople*, 11. Perhaps he is including the 2,000 marks borrowed from Venetian bankers by the crusading envoys after the conclusion of the treaty. Villehardouin, *Conquête de Constantinople*, 1: 32.

⁷⁴ Bernardo Maragone, *Annales Pisani*, ed. by M. Lupo Gentile, in *R.I.S.*², 6, pt. 2: 7. Runciman, *History of the Crusades*, 1: 299–300.

1099 lingered at Zara, for what reasons we are not told, and it fought Daimbert's fleet later in the Cyclades before the Venetians finally arrived in Palestine.⁷⁵ A later Venetian armada raided Corfu on its way to the battle of Ascalon and the capture of Tyre in 1124.⁷⁶ Richard the Lion Heart is well known, of course, for his extracurricular exploits in Sicily and Cyprus. In any case, Dandolo was correct in his assertion that the voyage to Egypt so late in the year was impractical.⁷⁷ Given the cruel situation of the crusaders on the Lido and the dangers to Venice from this large foreign army, it was a reasonable course to get under way for the interim destination of Zara, where supplies would be plentiful, and to winter there until the Mediterranean would again be safe for sailing.⁷⁸ The dictates of necessity coincided with Venetian self-interest in reacquiring Zara.⁷⁹

The delta of the Nile was a legitimate objective upon which the crusaders had had their eyes for at least forty years. In the 1160s King Amalric of Jerusalem led five unsuccessful expeditions there.⁸⁰ Renaud of Chatillon, lord of Transjordan, incensed Saladin in 1183 by his foray into the Red Sea area.⁸¹ Richard I of England had planned an expedition to Egypt as part of his crusade.⁸² Some fifteen years after the Fourth Crusade, the delta was invaded by the army of the Fifth Crusade and later was the scene of St. Louis' first crusade. Western strategists realized that the crusading states would always be in danger as long as the Muslims were able to mount an attack from Egypt upon their precarious position. If the crusaders could split Islamic power by a conquest of Egypt, on the other hand, Jerusalem would easily fall into their hands.⁸³

Other factors too tended to favor the Egyptian goal. In the first place, the Levantine Franks did not want a crusade in the Holy Land. The zealous

⁷⁵ *Translatio Sancti Nicolai*, 256–8.

⁷⁶ *Historia ducum Veneticorum*, ed. by H. Simonsfeld, *M.G.H.*, SS., 14: 73.

⁷⁷ Robert of Clari, *Conquête de Constantinople*, 12. The Venetian fleet for Constantinople, according to an act of 1255, was supposed to have departed by August 15. It wintered abroad. Lane, *Venice*, 69–70.

⁷⁸ Robert of Clari, *Conquête de Constantinople*, 11–2. Riant has charged the Venetians with deliberately delaying the arrival of the fleet in Zara, so that it would be too late to sail overseas. "Innocent III, Philippe de Souabe, et Boniface de Montferrat," *Revue des questions historiques*, 17 (1875), 369. He overlooks the announcement prior to sailing that they would winter at Zara. He was also unaware that the time between departure from Venice and arrival at Zara was taken up with the gathering of supplies and the enlistment of men owed by the various Venetian dependencies in the northern Adriatic. Manfroni, *Storia della marina italiana*, 1: 317–18.

⁷⁹ We do not subscribe to Cessi's view that the crusaders were not to participate in the attack on Zara, which was strictly a Venetian undertaking. "Venezia e la quarta crociata," 24, n. 1 and 27, and "L'eredità di Enrico Dandolo," 6, n. 1. Responsible crusading leaders could not have failed to recognize that they could not accompany the Venetians to Zara without becoming involved. We know of no scholar who has accepted Cessi's view.

⁸⁰ William of Tyre, *Historia rerum in partibus transmarinis gestarum*, in *Recueil des historiens des croisades, historiens occidentaux*, 1: 890–4, 902–13, 948–58, 962–9, 971–9.

⁸¹ Runciman, *History of the Crusades*, 2: 436–7. He dates the expedition to 1182, although other authorities give 1183. Hamilton A. R. Gibb, "The Rise of Saladin," in Setton, ed., *History of the Crusades*, 1: 582; Joshua Prawer, *The Crusaders' Kingdom: European Colonialism in the Middle Ages* (New York, 1972), 28. Ernoul-Bernard is the only western source to mention the expedition. Ernoul and Bernard le Trésorier, *Chronique*, ed. by Louis de Mas Latrie (Paris, 1871), 69–70.

⁸² Imperiale, ed., *Codice diplomatico*, 3: 20–2.

⁸³ J. J. Saunders, *Aspects of the Crusades* (Christchurch, New Zealand, 1968), 38.

Renaud of Dampierre, who by-passed the rendezvous at Venice in favor of a direct passage from Apulia to Syria, discovered that the Kingdom of Jerusalem was living under a truce with its infidel neighbors and had no desire for war-minded newcomers to upset the tenuous *modus vivendi*.⁸⁴ Another factor contributing to the decision to attack Egypt was the famine that had fallen over the Nile region for five years.⁸⁵ It certainly must have diminished Egypt's defensive strength. Tessier argued that the scarcity of food would have deterred the crusaders from an Egyptian campaign, but he seems to have forgotten that the crusaders were to be supplied by the Venetians in contrast to the usual method of living off the land.⁸⁶ The Egyptian famine, therefore, made the country a promising target for Western attack.⁸⁷

Unfriendly critics have accused the Venetians of diverting the crusade from this legitimate goal to Christian Constantinople for the sake of protecting their Egyptian commerce. The chronicle of Ernoul and Bernard the Treasurer, a source from the Latin Kingdom, claimed that the Egyptian government negotiated with the Venetians to this end.⁸⁸ The editor of the chronicle, Louis de Mas Latrie, picked up the argument,⁸⁹ and a few years later Carl Hopf appeared to have discovered the treaty by which Venice agreed to divert the crusade.⁹⁰ In 1877, however, Gabriel Hanotaux showed that Hopf's documentation consisted of well-known commercial treaties that could not be dated prior to the Fourth Crusade.⁹¹ Although there has been considerable reluctance to abandon the idea of collusion between Venice and the sultan,⁹² it does not have a sound foundation.

Actually, Venice stood to profit enormously from a Latin conquest of Alexandria. Egypt was a very important center for medieval Mediterranean commerce. The land was at once the terminus and the distribution point for the exotic goods from the Orient via the Red Sea. At Cairo the major trade artery divided into three branches: one branch followed the North African coastline to Tunis, Tripoli, and Mehadia on to Muslim Spain and Sicily; the second went to Alexandria, where goods were transported to Ascalon, Acre, Tyre, Tripoli, Lattakiah, or Constantinople; the third route went overland

⁸⁴ Villehardouin, *Conquête de Constantinople*, 1: 56; Ernoul-Bernard, *Chronique*, 340-1; Gunther of Pairis, *Historia*, 70.

⁸⁵ Gunther of Pairis, *Historia*, 71.

⁸⁶ Jules Tessier, *La quatrième croisade: la diversion sur Zara et Constantinople* (Paris, 1884), 48.

⁸⁷ The famine was followed by a devastating plague in 1201 and 1202, but this calamity was in the future after the agreement to attack Egypt had been made. It has been reported that the plague was the second greatest demographic disaster in Egypt's medieval history. E. Ashtor, *A Social and Economic History of the Near East in the Middle Ages* (Berkeley, 1976), 238. Is it possible that the plague became a factor in the later decision to go to Constantinople? There is no evidence.

⁸⁸ Ernoul-Bernard, *Chronique*, 345-6.

⁸⁹ Mas Latrie, *Histoire de l'île de Chypre*, 1: 161-4.

⁹⁰ Carl Hopf, *Geschichte Griechenlands vom Beginn des Mittelalters bis auf unsere Zeit*, in Ersch-Gruber, *Encyclopädie*, vols. 85-6 (1867-8), 188.

⁹¹ Gabriel Hanotaux, "Les Venitiens ont-ils trahi la Chrétienté en 1202?" *Revue historique*, 4 (1877), 74-102. The facts are published in Tafel and Thomas, eds., *Urkunden*, 2: 184-93.

⁹² Runciman, not unaware of Hanotaux's article, still appears reluctant to give up a Veneto-Egyptian plot. *History of the Crusades*, 3: 113-4 and n. 2.

across the Sinai peninsula to the Islamic cities of the Syro-Palestinian interior.⁹³

The disadvantage to Western merchants in obtaining their precious goods from the Orient in such intermediate markets as Sicily, Constantinople, or the Christian Levantine ports was, of course, the payment of an inflated price. Using quotations of pepper prices in the Cairo Geniza documents, Goitein has demonstrated that prices doubled between Cairo and other points along the trade routes, and he estimates that as much as 80 percent of the increase was pure profit for Egyptian merchants.⁹⁴ For a long time, however, the Latins had to be content with picking up the products of the East in these intermediate ports.

In the late twelfth century the merchants of Pisa, Genoa, and Venice opened a significant direct trade with Egypt, although it was still a small part of the total overseas commerce. We know that the Pisans, in spite of a papal ban, were supplying the Egyptians with war material in 1154.⁹⁵ For Genoa and Venice we have some quantitative evidence. Prawer reports that the notarial records of the Genoese Giovanni Scriba list 58 voyages to Egypt between 1154 and 1164 out of a total of 335 contracts, i.e., 17.3 percent.⁹⁶ Louise Buenger Robbert has kindly provided us with the following figures based on published Venetian commercial documents. Of 247 surviving twelfth-century contracts for Venetian business in the Eastern Mediterranean, only thirty concerned Egypt, or 12.3 percent.⁹⁷ Attention might also be drawn to the papal grant of the Venetian request in 1198 that the Republic might carry on trade with Muslim Egypt in nonstrategic goods.⁹⁸

Although the Venetians, at least, had certainly engaged in direct trade with Egypt since the ninth century, when two merchants from the lagoons pilfered the body of Saint Mark, it was Saladin who really opened his land to western merchants. Part of the sultan's overall policy of strengthening Egypt was to rebuild his navy to its Fatimid splendor, but Saladin's plans depended upon the importation from the West of ship-building materials scarce in Egypt, such as wood, pitch, and iron. To entice Italian merchants to bring him naval supplies, and to draw their attention away from the crusader states, he offered them trading concessions.⁹⁹ The Pisans signed a commercial agreement with

⁹³ Heyd, *Histoire du commerce*, 1: 378–84; S. D. Goitein, *A Mediterranean Society: The Jewish Communities of the Arab World as Portrayed in the Documents of the Cairo Geniza* (Berkeley, 1967), 1: 212; S. Y. Labib, *Handelsgeschichte Ägyptens im Spätmittelalter (1171–1517)*, in *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, Beiheft (Weisbaden, 1965), Karte I; Adolf Schaube, *Handelsgeschichte der romanischen Völker des Mittelmeergebiets bis zum Ende der Kreuzzüge* (Munich and Berlin, 1906), 22.

⁹⁴ Goitein, *Mediterranean Society*, 1: 222.

⁹⁵ Robert-Henri Bautier, *The Economic Development of Medieval Europe*, trans. by Heather Karolyi (London, 1971), 101.

⁹⁶ Prawer, *The Crusaders' Kingdom*, 399.

⁹⁷ Her figures are based on Morozzo della Rocca and Lombardo, eds., *Documenti del commercio veneziano*, and Morozzo della Rocca and Lombardo, eds., *Nuovi documenti del commercio veneto*.

⁹⁸ Tafel and Thomas, eds., *Urkunden*, 1: 234–5.

⁹⁹ A. S. Ehrenkreutz, "The Place of Saladin in the Naval History of the Mediterranean Sea in the Middle Ages," *Journal of the American Oriental Society*, 75 (1955), 100–1; Gibb, "Rise of Saladin," 583–4; Roberto Cessi, *Le colonie italiane medievali in Oriente* (Bologna, 1942), 1: 75–6.

Saladin in 1173 that gave them free trade and security throughout Egypt; additional negotiations in 1177 and 1179 further enhanced their position.¹⁰⁰ The Venetians gained their privileges in 1175,¹⁰¹ and the Genoese obtained theirs in 1177.¹⁰² Saladin was therefore able to boast to the caliph in Baghdad that "treaties of peaceful intercourse have been negotiated with them all."¹⁰³

The embryonic Latin establishments at Alexandria, however, were quite precarious. The westerners had recourse neither to friendly political authority nor to religious sanctions if they fell afoul of the Muslim government. We have considerable evidence of their troubles in Egypt. In 1195 the Egyptians released some Venetians who had been held captive.¹⁰⁴

It appears that the Pisans also had difficulty, for an Egyptian edict from the end of the twelfth century restored the Pisan *fondaco* at Alexandria and promised to protect their goods and privileges there.¹⁰⁵ In 1200 the Genoese tried unsuccessfully to obtain the release of some of their fellow citizens imprisoned by the sultan.¹⁰⁶ A few years after the Fourth Crusade in 1208, Pisan and Venetian merchants whom al-Adil had detained were requesting that the sultan allow them to return home.¹⁰⁷

Although Venice improved its position in Egypt a few years after the Fourth Crusade by concluding a series of commercial agreements with the Ayubbid government,¹⁰⁸ the city nevertheless would have gained much more from a successful crusade against the delta. These treaties contained little more than confirmation of existing property rights and the customary vague promises of security for Venetian merchants in Egypt except for the gain of a second *fondaco* and a Venetian *podestà* at Alexandria.¹⁰⁹ The treaty with the crusaders, however, allocated to the Venetians one-half of all the conquests as their own colonies.¹¹⁰ This provision would have entailed, of course, control over taxation, Venetian courts and law, and whatever else political authority might do to serve the interests of Venetian merchants.

Had the Venetians gained such a colony in Egypt, they could have siphoned off the lucrative trade via the Red Sea where it first entered the Mediterranean area and where goods were cheapest. They could have consolidated their

¹⁰⁰ Reinhold Röhricht, ed., *Regesta regni Hierosolymitani* (Innsbruck, 1893-1904), 1: 132, 144, 152; Cessi, *Coloniae mediaevali*, 1: 77.

¹⁰¹ *Historia ducum veneticorum*, 81.

¹⁰² L. T. Belgrano and Cesare Imperiale di Sant' Angelo, eds., *Caffari et continuatorum Annales Januenses* (Rome, 1890-1901), 2: 11.

¹⁰³ Quoted by Gibb, "Rise of Saladin," 584. Curiously, Benjamin of Tudela, a Jewish traveler in Egypt during the early years of Saladin's reign, does not include the Venetians in his list of Italian merchants doing business there. Although this omission does not prove that Venetians were not trading there, it does suggest that they did not play a dominant role among the European traders. Benjamin of Tudela, *The Itinerary*, ed. and trans. by A. Asher (London, 1840-1), 1: 157. Benjamin does mention "Valencia," which some think may be a corruption of "Venetia," although this conjecture seems to us a quite arbitrary emendation.

¹⁰⁴ Labib, *Handelsgeschichte Ägyptens*, 29.

¹⁰⁵ Röhricht, ed., *Regesta regni Hierosolymitani*, 1: 204-5. The document cannot be dated precisely.

¹⁰⁶ Belgrano and Imperiale, eds., *Annales Januenses*, 2: 79.

¹⁰⁷ Röhricht, ed., *Regesta regni Hierosolymitani*, 1: 223.

¹⁰⁸ *Supra*, p. 729 and n. 91.

¹⁰⁹ Tafel and Thomas, eds., *Urkunden*, 2: 189.

¹¹⁰ *Ibid.*, 1: 367.

trade there and rested easy from the unsettled political conditions affecting the city's far-flung entrepôts used in the twelfth century to gather goods from the East. The agreement with the northern crusaders to direct the Fourth Crusade toward Egypt, therefore, offered extremely enticing prospects to Venice.

The theory that Venice diverted the Fourth Crusade to Constantinople is based largely on the belief that the Byzantine capital had an overwhelming economic importance for the city on the lagoons. While one cannot doubt the crucial role that Byzantium played in Venice's commercial expansion during the eleventh and twelfth centuries, one can question whether the Queen of the Bosphorus still enjoyed unchallenged commercial supremacy on the eve of the Fourth Crusade and whether the Venetians, in fact, sought their commercial advantage by her conquest.

In the first place, while Byzantium reaped the fruits of its advantageous geographical position as an entrepôt for interregional commerce, it did not enjoy an overwhelmingly important industrial economy. In fact, one searches almost in vain to find anything produced or manufactured within the empire itself that was salable in quantity on the world market. It produced gold and silver articles, which were too expensive for volume trade; armaments, which by their very nature were not for sale to rivals; and silk garments, whose manufacture, sale, and export were strictly regulated by the state, so that silk could not contribute much to the overall economy.¹¹¹ In short, Byzantine wealth did not result from widespread industry.

Constantinople's economic prosperity had always rested upon its role as the marketplace of the Mediterranean. To the shores of the Bosphorus came the goods of the Islamic lands and the exotic Orient beyond for transshipment to the West. At the end of the twelfth century, however, the city's domination of interregional trade had weakened. We have no quarrel with M. F. Hendy's revisionist thesis that in the eleventh and twelfth centuries the Byzantine economy was expanding rather than declining.¹¹² Hendy treats the period en bloc, while we are concerned here with the years after the Third Crusade relative to the rest of the twelfth century. Hendy agrees with other economic historians, moreover, that the Italians dominated and exploited Byzantine commerce in the twelfth century.¹¹³ Once Constantinople's position had been weakened by the rise of other Levantine ports, its usefulness to the Italians diminished.

The establishment of the crusader states drained off a considerable amount

¹¹¹ Runciman, "Byzantine Trade and Industry," 103-7. See also Robert S. Lopez, "Silk Industry in the Byzantine Empire," *Speculum*, 20 (1945), 1-42.

¹¹² M. F. Hendy, "Byzantium, 1081-1204: An Economic Reappraisal," *Transactions of the Royal Historical Society*, ser. 5, 20 (1970), 31-52. Actually Hendy makes a number of points that tend to support our view. For an extreme expression of the traditional position, see Thompson, *Economic and Social History of the Middle Ages*, 414.

¹¹³ On the continued existence of commerce carried by Byzantines in the twelfth century, see Hendy, "Byzantium," 40, and Runciman, "Byzantine Trade and Industry," 102-3. We see no reason, nevertheless, to consider invalid the generally held view that Italians dominated the interregional trade of the later twelfth century.

of business from the once-proud Byzantine emporium. Many of the goods traded at Constantinople came through what were in the twelfth century crusader ports. Antioch (or more specifically, its ports of Lattakiah and St. Simeon) had become especially important, serving as the Mediterranean outlet for goods coming up the Euphrates to Aleppo from Baghdad and the East. Tyre and Acre both received wares from the East distributed at Damascus, besides possessing their own thriving industries.¹¹⁴ Antioch, Tripoli, Tyre, and Acre were now in the hands of western barons who had granted liberal concessions, indeed, in the course of the Third Crusade, virtual autonomy, to the Italians, whose sea power protected the fragile Latin kingdom.

Genoa, in return for giving aid to the residual crusader forces defending Tyre against Saladin, received from Conrad of Montferrat extensive property, tax exemptions, and a court at Tyre.¹¹⁵ King Guy at the siege of Acre, apparently in an attempt to draw support away from his rival, Conrad, as much as to assure the Italians' continued support of the siege, confirmed Genoa's pre-Saladinic possessions at Acre and included exemption from taxes.¹¹⁶ In order to outdo Guy, Conrad later added an extraterritorial court at Acre to Genoa's privileges.¹¹⁷ Henry of Champagne, regent of the kingdom, ratified these agreements for Tyre in 1192 and for Acre in 1195.¹¹⁸

Much the same story holds for the Pisans. Conrad of Montferrat granted Pisa its pre-Saladinic possessions in the kingdom in 1187 and 1189 along with some additional property and a 2,000 bezant annuity from the port and market dues at Acre.¹¹⁹ King Guy, allied with the Pisans against Conrad and the Genoese, confirmed Conrad's grant to the Pisans.¹²⁰ Conrad reconfirmed Pisa's Tyrian possessions in 1191.¹²¹ Finally, Henry of Champagne in 1193 confirmed once more all Pisa's properties and privileges and added a tax exemption at Acre.¹²²

By the end of the twelfth century, moreover, the Armenian national leader, Roupen, had carved out a kingdom in southern Cilicia independent of Byzantium and hostile to her.¹²³ Italian merchants flocked to the new Armenian court to obtain trading concessions in the kingdom. In March 1201, the Genoese gained liberal commercial privileges at the ports of Mamistra, Tarsus, and Sis,¹²⁴ and in December the Venetians followed suit.¹²⁵ These two Italian cities were thus able to capture the commerce coming into the Medi-

¹¹⁴ Heyd, *Histoire du commerce*, 1: 164-182; Prawer, *The Crusaders' Kingdom*, 391-7; Thompson, *Economic and Social History of the Middle Ages*, 404.

¹¹⁵ Imperiale, *Codice diplomatico*, 2: 369-72.

¹¹⁶ *Ibid.*, 2: 374-6; 3: 22-3.

¹¹⁷ *Ibid.*, 3: 48-50.

¹¹⁸ *Ibid.*, 3: 87-9, 113-5.

¹¹⁹ Röhrich, ed., *Regesta regni Hierosolymitani*, 1: 177, 178, 180.

¹²⁰ *Ibid.*, 1: 182.

¹²¹ *Ibid.*, 1: 188.

¹²² *Ibid.*, 1: 191.

¹²³ Sirarpie der Nersessian, "The Kingdom of Cilician Armenia," in Setton, ed., *History of the Crusades*, 2: 643-4.

¹²⁴ *Libri iurum rei publicae Genuensis*, vol. 1, ed. by Ercole Ricotti, in *Historiae patriae monumenta*, 7: 468-70.

¹²⁵ Tafel and Thomas, eds., *Urkunden*, 1: 381-5.

terranean through Armenian ports that had before been shipped by sea or overland to Constantinople for world purchase.¹²⁶

Cyprus had always been a focal point for the trade of the eastern Mediterranean, lying as it did on the crossroads of commerce coming from the Syrian littoral or Egypt to Constantinople. In 1191 Richard the Lion Heart conquered the island and sold it to the house of Lusignan as a lordship, which later became a kingdom in 1197.¹²⁷ This kingdom provided the Italians with another friendly location where they could establish alternative trading centers to Constantinople.

To sum up the diplomatic and commercial situation in the eastern Mediterranean for the period immediately prior to the Fourth Crusade, the Italian cities, and especially Genoa and Pisa with their profitable exploitation of the Third Crusade and its factions, were gaining control of the sources of the trade upon which Constantinople's economic well-being depended. Hendy has remarked: "By its very geographical situation Constantinople straddled a main north-south trade axis, and as long as the Empire held most or all of the strategic ports of south Asia Minor, Cyprus, Crete, and the southern Peloponnese, it stood to derive similar benefits from the main east-west one."¹²⁸ By the end of the twelfth century, however, Byzantium no longer exercised such hegemony, and new entrepôts for the trade from the East had arisen on the Syrian coast. It is conceded that at this early date the empire's Russian and Black Sea trade remained flourishing and free of Italian domination. Venice had the strongest position in Constantinople, but it was at the very moment when the Byzantine capital was fast losing commercial advantage. Far from trying to plunder a sinking ship, the Venetians needed to transship to a sturdier vessel, or at any rate, to diminish their reliance upon the one that was listing.¹²⁹ An argument, therefore, can be made that the Venetians would have preferred a successful expedition to Egypt over one to Constantinople.

Another well-known argument of the anti-Venetian school, derived largely from the Byzantine chronicler Nicetas Choniates, is that Venice intended to divert the crusade to Constantinople because the emperors had been consistently favoring Genoa and Pisa as counterweights to Venetian commercial

¹²⁶ Heyd, *Histoire du commerce*, 1: 367.

¹²⁷ Sidney Painter, "The Third Crusade: Richard the Lionhearted and Philip Augustus," in Setton, ed., *History of the Crusades*, 2: 62-4, 81-2; Edna Chapin Furber, "The Kingdom of Cyprus, 1191-1292," in Setton, ed., *History of the Crusades*, 2: 600-2, 604.

¹²⁸ Hendy, "Byzantium," 40.

¹²⁹ To illustrate how much Byzantine trade dominated the commerce of Venice in the twelfth century, we again rely upon Robbert's figures. *Supra*, n. 97. Out of 247 contracts, 170, or more than two out of three, involved Constantinople or some place else in the Byzantine Empire. Broken down into the periods 1150-1183 and 1184-1205, the figures show Byzantine Empire, 100 and 60, respectively; crusader states and Egypt, 47 and 32, respectively. For the first period 29 percent of Venice's commerce was non-Byzantine and for the second period 35 percent. Venetian commerce with the Levant outside of Byzantium, therefore, constituted only a small part of her total commerce and did not radically increase relative to that with Byzantium. The contracts concerning Egypt make up only 13 percent of the total for 1150-1183 (20 out of 155) and 11 percent for 1184-1205 (10 out of 92). Thiriet, with quite a different intention, has pointed out how very few Venetian contracts for commerce in the Byzantine Empire there were at the end of the twelfth and the beginning of the thirteenth centuries. *Romanie vénétienne*, 61, n. 1.

power. Fearing competition, the Venetians sought to gain monopoly by conquest.¹³⁰ A consideration of Venetian policies toward the Pisans and the Genoese in the capital after its conquest casts grave doubts upon this thesis. The pact concluded between the Venetians and the crusaders concerning the division of the spoils, if they should conquer Constantinople, does not name any group of merchants to be expelled. The treaty stipulates only that anyone at war with Venice is to be excluded from the empire until peace has been made.¹³¹ This clause is an example of the use of commercial leverage for political and military ends, rather than the contrary. Although it cannot be proved positively, the Genoese were probably expelled,¹³² but the cause of this measure was the skirmishing and near war that arose between Venice and Genoa concerning the disposition of Crete. Possession of the island fell to Boniface of Montferrat, who had intended to sell it to the Genoese, until the Venetians, anticipating the sale, purchased it from him.¹³³ The dismayed Genoese gave considerable support to a native Cretan noble, Count Henry Piscatore of Malta, who plagued Venice for several years in his attempt to win Cretan independence.¹³⁴ What is remarkable, however, is that in 1218, when Genoa and Venice patched up a treaty over Crete, Venice granted Genoa not only re-entrance into the empire, but also all the privileges the Genoese had enjoyed under Alexius III, precisely the status quo immediately prior to the conquest of Constantinople.¹³⁵

The Pisans, whom Nicetas names as rivals of Venice especially favored by Alexius III,¹³⁶ never lost their rights in the empire.¹³⁷ True most of the Pisan facilities had been destroyed in the fires accompanying the Latin conquest,¹³⁸ but the Constantinopolitan Pisans, who had fought staunchly against the crusaders and the Venetians, saved themselves from expulsion by joining the attackers on the eve of the final assault.¹³⁹ It does not appear that Venice had a concerted or permanent policy of driving out competition; its main concern was to protect itself from temporary political enemies.

Even the argument employed by the anti-Venetianists that Dandolo and his people were pursuing a policy of revenge and were seeking restitution for the seizure of their goods and persons in the empire in 1171 can be challenged.¹⁴⁰ There is no doubt that the Venetians suffered terribly, but an agreement for compensation had been reached in 1187,¹⁴¹ and the reparations had been

¹³⁰ Nicetas Choniates, *Historia*, 712-4.

¹³¹ Tafel and Thomas, *Urkunden*, 1: 448, 452.

¹³² Heyd, *Histoire du commerce*, 1: 292.

¹³³ J. K. Fotheringham, "Genoa and the Fourth Crusade," *English Historical Review*, 25 (1910), 38-41.

¹³⁴ Belgrano and Imperiale, *Annales Januenses*, 2: 100-1, 109-10, 114. *Historia ducum veneticorum*, 95, n. 3.

¹³⁵ Tafel and Thomas, eds., *Urkunden*, 2: 202-5.

¹³⁶ Nicetas Choniates, *Historia*, 713.

¹³⁷ Fotheringham says, probably erroneously, that the Pisans were expelled. "Genoa and the Fourth Crusade," 34. Heyd disagrees. *Histoire du commerce*, 1: 290. See also Schaube, *Handelsgeschichte*, 269.

¹³⁸ Nicetas Choniates, *Historia*, 730.

¹³⁹ *Ibid.*, 730-1.

¹⁴⁰ *Historia ducum veneticorum*, 78-80; Dandolo, *Chronica*, 250-1.

¹⁴¹ Tafel and Thomas, eds., *Urkunden*, 1: 206-11.

almost fully paid.¹⁴² The Venetians had not only regained their position in the empire by a series of chrysobulls from Andronicus and Isaac II,¹⁴³ but the one issued by Alexius III in 1199 gave them a situation better than the one they had hitherto enjoyed. It indicates the extensive penetration of the Venetians into the economic life of the empire and establishes those places in which imperial jurisdiction was limited. Venetian freedom in the Byzantine Empire was beginning to approach the favorable situation the Italians enjoyed in the crusader states, where they seem to have been practically immune from local authorities.¹⁴⁴

The treaty of mutual assistance of 1203 between the Venetians and the crusaders, on the one hand, and German envoys on behalf of the young Alexius, the refugee Byzantine prince, on the other, also fails to show the Venetians' interest in improving their position in Constantinople. The terms were as follows: in return for Venetian help in deposing his uncle, Alexius III, Alexius promised to submit the Greek Church to Rome, to pay 200,000 silver marks, to provide provisions for the crusading army, to join the crusade or to send 10,000 men for a year, and to maintain 500 knights in the Holy Land for the duration of his life.¹⁴⁵ Hugh of St. Pol specifies that half of the 200,000 marks would be received by the Venetians,¹⁴⁶ the distribution that would follow from the treaty of 1201. There is no evidence that the Venetians demanded anything more, and the main thrust of the agreement is clearly to aid the Church and the crusade. In fact, after Alexius III had been deposed the Venetians claimed no new privileges in the empire from the new emperor. It was at the request of Alexius IV that the crusaders and their fleet remained at Constantinople after his coronation.¹⁴⁷ Only the delays and the ultimate outright refusal by Alexius to fulfill the promises he had made in the treaty of 1203 prevented the crusaders and the Venetian fleet from proceeding with their intention of making Holy War.

It has not been our intention to whitewash the Venetians, to contend that their motives were pure, or to deny that they saw commercial advantage in the installation of a dependent young refugee upon the Byzantine throne. They were probably as greedy as feudal barons or pious prelates, or for that matter, modern corporate technocrats or teachers of history; no more, no less. By

¹⁴² For the clearest account of the history of the Byzantine indemnification to the Venetians, see Charles M. Brand, *Byzantium Confronts the West, 1180-1204* (Cambridge, Mass., 1968), 199-200. The question remains somewhat confused, but it is clear that payment was at least almost complete, and Dandolo's willingness in 1197 to accept less than full payment and the absence of any mention in the chrysobull of 1199 make one doubt whether nonpayment could have been a cause for Venetian aggression against the emperor.

¹⁴³ Tafel and Thomas, eds., *Urkunden*, 1: 175 (Andronicus, 1184), and 1: 178-203 (Isaac II, 1187). The result was a confirmation of the Venetian position as it had existed before the debacle of 1171.

¹⁴⁴ *Ibid.*, 1: 246-78. While Horatio F. Brown mentions "new privileges" contained in this chrysobull, historians in general have failed to note the significance of Alexius III's concessions. "The Venetians and the Venetian Quarter in Constantinople to the Close of the Twelfth Century," *Journal of Hellenic Studies*, 40 (1920), 88. For Venetian extraterritorial privileges in the crusader states, see Marsilio Zorzi's report of 1243. Tafel and Thomas, eds., *Urkunden*, 2: 351-98.

¹⁴⁵ Villehardouin, *Conquête de Constantinople*, 1: 92-4. The treaty itself is not extant.

¹⁴⁶ Hugh of St. Pol., *Epistola*, in *M.G.H.*, SS., 17: 812.

¹⁴⁷ Villehardouin, *Conquête de Constantinople*, 1: 196-200.

presenting some arguments contrary to those generally heard, however, we have aimed at redressing the one-sided view that pictures the Venetians as the villains of the conquest of Christian Constantinople. Bourgeois as they were, the Venetians possessed an honorable crusading tradition and shared the religious ideals of the northern knights. When their allies proved unable to pay for the contracted fleet, the Venetians were compelled to find a way to obtain compensation and to prevent the dissolution of the crusade. At the very end of the twelfth century and the beginning of the thirteenth, moreover, Alexandria possessed an increasing attraction for Venetian merchants which made the plan to attack it good commercial as well as military strategy. In the Byzantine capital the Venetians still held the most favored position, but the domination of Levantine trade by Constantinople was being challenged by the Italian colonies in the crusader states and in Egypt. All in all, we believe that these arguments call for a reappraisal of the Venetian role in the Fourth Crusade. They indicate, at least, that the widely held view of a cynical Venetian betrayal of the crusade for the sake of profit is one-sided, simplistic, and untenable.

The Right to Rule in England: Depositions and the Kingdom's Authority, 1327–1485

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A SERIES OF FIVE DEPOSITIONS between 1327 and 1485 drove the English to conjure up arguments and principles justifying a change in kings. Five sovereigns—Edward II, Richard II, Henry VI, Edward V, and Richard III—lost the throne permanently; and four usurpers—Henry IV, Edward IV, Richard III, and Henry VII—claimed to be “lawfully seised and possessed of the said crown” in their stead. To sanction the removal of an unsatisfactory sovereign and his successor’s usurpation, men of law—both common and canon—devised apologies. These justificatory declarations, taken seriatim, created a doctrine of restraint upon the regal power that eventually became a part of England’s constitutional, or public, law.

On each of these occasions, God and His Englishmen had the chance to choose a new king. As they did so, their spokesmen defined further the right to rule and the authority to govern, thereby limiting the English monarchy, for to define is to curb. The deponents, to justify their deeds, alleged deceit, deviousness, distrust, and “uselessness” in those they removed; and sometimes irresponsibility, bad faith, and a breach of the coronation oath. The deposed kings had governed, they said, without good counsel and consent, or against England’s laws and customs. Positively, the usurpers claimed the crown through inheritance or by God’s will, conquest, or election. Thereby they brought into play and into public law God’s authority, the people’s, and the kingdom’s—authority that was expressed through the three estates of the realm and, finally, through parliament. These indictments of beaten kings and the vindications of their conquerors contained much bad history. In due season they made good law.

Again and again the official apologies raised basic questions about public law and governance. To whom was, or ought, the good king to be responsible? And for what and how much was he himself to be answerable, and to whom? To God alone? Yet that position was hardly satisfactory. To medieval men, the kingdom possessed authority of its own that in several ways complemented God’s, thus suggesting that the king was, in some measure, also

responsible to it and its people. Already in the thirteenth century men of law and politics were writing about the community (or communities) of the realm. Before 1399, drawing from the “estates literature,” they had begun to use the phrase, “the estates of the realm,” to designate the grades (*gradus*) in the hierarchy of society that were taken to be the components of the kingdom. This new term, estate, was to prove useful for depositional apologias, and in 1398 it was joined by another new one, “the authority of parliament,” which eventually absorbed the kingdom’s authority—and the people’s too. Englishmen did not immediately resolve all the constitutional questions raised by the removal of a king, nor did they do so in 1689 when they erected the modern monarchy on medieval foundations that the Glorious Revolution refurbished. Yet at each deposition, their actions created precedents for future ones, and in their attempts to justify what they had done, they established and then reiterated principles that were, in the end, to form a constitutional doctrine legitimating a right to depose and a right to rule.

To decide just when and how to depose an erring monarch was the highest decision England’s governing class had to make. Furthermore, those initiating a deposition had also to persuade a large majority of the men of power to go along with their decision. Otherwise the successor or usurper laid himself open, as did Edward IV and Richard III, to his own removal. For 164 years after 1485 not a single attempt at usurpation or deposition succeeded until Charles I’s beheading in 1649; and after James II fled in 1688, no monarch was forced to leave the British throne for 248 years, until Edward VIII’s deposition in 1936 drove him into exile.

The formula that men used to justify Edward II’s removal in 1327 was brief and simple and in close conformity to law and precedent. Edward III’s proclamation at his accession declares that his father “ousted himself from the government of the said realm and has granted and willed that the government of the said realm devolve upon Sir Edward, his eldest son and heir, and that he govern, reign, and be crowned king.” Edward II, “formerly king of England,” had done this “of his good will and by common counsel and assent of the prelates, earls, and barons, and other nobles, and the whole community of the kingdom.” This neat formula rested upon two principles: first, the common law of inheritance by the eldest son, and then the century-old rule that the king acts on matters affecting the kingdom with the counsel and consent of the prelates, magnates, and community of the realm. Hostility toward Edward II personally was widespread, and very few, not even his half-brothers or his son, seemed to care about the wretched man. None would fight on his behalf, perhaps “because of his cruelty and personal faults,” and his subjects were ready to give him up as a “useless king” (*rex inutilis*), one politically inept and personally incompetent.¹

¹ S. B. Chrimes and A. L. Brown, eds., *Select Documents of English Constitutional History, 1307–1485* (London, 1961), 38 (no. 24). For the *rex inutilis*—the useless, incompetent, inept, weak, and incorrigible king—and the connection between this concept and early medieval depositional theory, see Edward Peters, *The Shadow King: Rex Inutilis in Medieval Law and Literature, 751–1327* (New Haven, 1970), *passim* and esp. 217, 232, 237–41;

Before staging the final denouement, a number of prelates, earls, barons, and knights met on October 26, 1326, at Bristol with Queen Isabella. "By the assent of the whole community of the said kingdom present there, they unanimously chose the duke [Edward III] as keeper of the said kingdom"—but not yet as king.² King Edward II fled to Wales with the young Despenser, but was captured on November 16 and imprisoned at Kenilworth. A parliament, first summoned to meet on December 14, was prorogued until January 7, 1327, when an assembly—a few chroniclers called it a parliament, others did not—met in Westminster Hall. The details are significant, for they set the pattern of procedures and arguments that would be followed in subsequent depositions.

On Tuesday, January 13, the archbishop of Canterbury read to "all the baronage of the land" six "reasons" why Edward III should succeed his father. One was that Edward II's "person . . . was not sufficient to govern." Another, that evil counselors had misled him, and he had not listened "to good counsel." Instead, he had given himself over to unsuitable pastimes and had neglected "the needs of his kingdom." Besides, he had lost Scotland and lands in Gascony and Ireland. He had injured the Church, imprisoned its clergy, and dilapidated the realm. Above all, Edward II had not wished to do right to all his subjects, "as he is bound by his oath to do"; nor had he "kept the other points of the oath that he took at his coronation." When the archbishop had finished presenting these charges, "all the people assented that he should reign no more, but that his eldest son, the duke of Guienne [Edward III], should reign and bear the crown for him." Thereupon the assembly of prelates and magnates, of knights and burgesses (a *parliamentum* according to one chronicle but a *concilium generale totius cleri et populi* in another) constituted the kingdom's representative and appointed a commission "from the part of the whole kingdom" to go to Edward II at Kenilworth. These commissioners, as representatives of the Westminster assembly, were to speak "for the community of the land" and to ask the king whether he "would assent to his son's coronation and resign the rule." If not, they were to "render up their homage for all the land."³

At Kenilworth, the bishops of Winchester and Lincoln and the earl of Lancaster spoke secretly with the king and advised him to "resign the crown to his first-born son." After prevaricating and "adulterating the word of truth" somewhat, as *Baker's Chronicle* admits, these emissaries "threatened that unless he [Edward II] resigned, the people . . . would exalt as king some one other than of the royal family." In the end, Edward II "answered that he greatly grieved that the people of his realm should be so exasperated with him that they loathed his rule," but he was pleased "that his son was accepted by

Peters makes it evident that English canonists were familiar with this doctrine by 1327. The most recent study of the problems of Edward II's reign, with bibliographic review, is Charles T. Wood, "Personality, Politics, and Constitutional Progress: The Lessons of Edward II," *Studia Gratiana*, 15 (1972) [*Post Scripta*]: 519–36.

² Chrimes and Brown, eds., *Select Documents*, 33 (no. 18).

³ *Ibid.*, 35, 37, 36, 34; May McKisack, *The Fourteenth Century, 1307–1399* (Oxford, 1963), 89–91.

the people.” The next day Sir Thomas de Blount, steward of the royal household, broke the virge as was done over a dead king’s grave. Sir William Trussell, for the commissioners, returned “the homages and allegiances for the whole kingdom to Sir Edward of Carnarvon, recently king.” Hereafter, his former subjects were to hold Edward, “father of the lord king who now is,” to be but “a private person without any kind of royal dignity.” Thereupon the commissioners went back to the “parliament at London” and “reported fully the king’s answer—nay, rather more fully than it was made.”⁴

In the meantime, the archbishop and two bishops had preached to the assembly in Westminster Hall. On Tuesday, January 13, the bishop of Hereford took for his text, “A foolish king shall ruin his people,” and in this sermon “he dwelt weightily upon the folly and unwisdom of the king, and upon his childish doings.” At the end, “all the people answered with one voice—‘we will no longer have this man to reign over us.’” The next day, the bishop of Winchester preached on the words, “My head pains me.” He then explained “with sorrow what a feeble head England had had for many years.” On the third day, January 15, 1327, the archbishop of Canterbury used as the text of his sermon those words so dangerous to a kingly rule, “*vox populi, vox Dei*,” to announce that “by the unanimous consent of all the earls and barons, and of the archbishops and bishops, and of the whole clergy and people, King Edward [II] was deposed from his pristine dignity, never more to reign nor to govern the people of England; and he added that all . . . , both laity and clergy, unanimously agreed that my lord Edward, his first-born son, should succeed.” Edward III’s accession was proclaimed in London on January 24, and his reign was dated from the 25th. A week later, February 1, he received unction and the crown. Thereafter, Edward II was moved from Kenilworth to Berkeley Castle to be kept safely “in perpetual prison” until September 1327 when the wretched man was “vilely murdered” in a most torturous way.⁵

Political success, rather than legal process, justified Edward II’s deposition. Nonetheless, the sponsors of the 1327 dethronement deemed it expedient to cite authority to sanction one king’s expulsion and another’s succession. To the formula that Edward II “had ousted himself from the government,” the victors added that he “was deposed . . . by the unanimous consent” of the lords and of “the whole clergy and people.” Likewise, the queen’s apologists claimed that Edward II had granted the crown to his son “by the common counsel and assent of the prelates,” of the peers, and of “all the community of the kingdom.” In this way they rooted the principles of counsel and consent still deeper in England’s laws and customs. Further, the statement, “it is agreed that Sir Edward [III], the king’s eldest son, should have the government of the realm and be crowned king,” seems grounded in the principle of contract. Nevertheless, the deponents still assumed that Edward III, his father’s “first-born” and “eldest son,” succeeded by right of inheritance upon

⁴ Chrimes and Brown, eds., *Select Documents*, 34, 35, 38.

⁵ McKisack, *Fourteenth Century*, 89; Herbert Maxwell, trans., *The Chronicle of Lanercost* (Glasgow, 1913), 254–58; Chrimes and Brown, eds., *Select Documents*, 36.

his father's demise. In terms of the crown, that event occurred in January, when Edward II was transformed from a public person into a private one, a change that made his body politic dead in public law even though the body natural lived on into September. As a result, Edward III did indeed acquire a right to rule when he succeeded his father's body politic, but one should also recognize that insofar as "only God can make an heir," as Glanville had taught, it was really God through His authority who transmitted this right to the kingdom. And the new king became God's vicar and vicegerent, too, when the archbishop of Canterbury hallowed him with oil and crown, the rite that gave a monarch mystique and thaumaturgy.⁶

At Edward III's coronation, as at others in the fourteenth century, the people (*plebs*) signified their consent (*consencientibus*) by shouting "with a great and unanimous voice," *fiat, fiat, and vivat rex*. Therewith they sanctioned the king with the kingdom's mystic authority, one that was expressed in writings of the time through the use of such concepts as the community or communities of the kingdom, the estates of the realm, or simply the people. Just what men meant by the word, *people*, in 1327 or later, is not certain, but probably it contained some of the connotations of the Roman *populus*, which "referred to a group of rational beings associated by some form of consent expressed in laws, policy, or public ritual." The estates of the realm was a concept applied in 1327 when the prelates and magnates went to the London Guildhall and swore their oaths of allegiance to the new king. The Guildhall record listed the names of those taking the oath according to their rank, order, and degree; and, depending on how one counts, these form eight to twelve categories. In common parlance as well as literary usage, these were "the estates of the realm," and while their number in 1327 was uncertain, in 1420 the Treaty of Troyes with France would reduce it permanently to three.⁷ In 1399, the Lancastrian apologists would pick up the phrase and give it currency, for it was through use of that term that they invoked the kingdom's authority to justify their deposition of Richard II, an anointed king from whom not even "the rude sea" could "wash the balm."

A deep distrust of the man, Richard Plantagenet, impelled Henry of Bolingbroke and his adherents to depose their king, but the Lancastrians also held different views on England's polity. In 1386 Richard II had quarreled with the autumnal parliament when it impeached the retiring chancellor, the earl of Suffolk, and forced upon the king a commission with power to dismiss his

⁶ *Ibid.*, 38, 37; *Lanercost*, 255; G.D.G. Hall, ed. and trans., *The Treatise on the Laws and Customs of the Realm of England, Commonly Called Glanvill* (London, 1965), 71; Francis Palgrave, ed., *Parliamentary Writs* (London, 1827-34), 1: 396 (no. 37); Barnaby C. Keeney, "The Medieval Idea of the State," *University of Toronto Law Journal*, 7 (1949): 65 and n. 50.

⁷ For discussion of the medieval meanings of the concept, *Populus* [People?], see Jeremy du Quesnay Adams, *The Populus of Augustine and Jerome* (New Haven, 1971), 2: "The consent involved was usually conscious and at least potentially subject to alteration; predetermining factors such as ethnic or linguistic affiliation were not closely or normally associated with it. . . . It was a word full of meaning and resonance, quite different from the diffuse and probably polysemic *people* which is less its heir than its descendant." Cf. 76, 109-19. In the fifteenth century, the three estates were the lords spiritual and temporal and the commons; centuries later they would become the king, lords, and commons.

servants. A few peers had threatened Richard with deposition, and the next year the king, not yet twenty-one, ordered the justices of the two benches to declare his rights. To them he put Ten Questions, and their answers provided this ruler with a rationale, if not a constitution, that authorized a concentration of power in the kingly office.⁸

Like their young king, the justices championed regality and declared that a 1386 statutory commission had infringed upon the king's "prerogative and the liberties of the crown." That statute, they said, was derogatory to Richard, "especially as it had been contrary to the king's wishes." Answers to the next questions stated the Ricardian view of parliament as the king's high court. Lords and commons were not to take up their matters first, nor first to require the king to assent to their decisions. Rather, "the king's articles ought to be proceeded with first" because "the king should have the rule of parliament." Moreover, the king might dissolve parliament whenever he pleased (as Richard's predecessors had done), and he could order the lords and commons to go away. "If anyone thereafter acts contrary to the king's will, as though he were in parliament," he should be punished as if a traitor. Nor should the lords and commons impeach (*impetere*) the king's officers and justices "contrary to the king's wish."⁹

The most dramatic of the Ten Questions, and politically the most inflammatory, was the ninth. How ought the man to be punished who had moved in the parliament of 1386 to "send for the statute by which King Edward [II] . . . had been adjudged in parliament?" The justices answered that this man and the one "who, under pretext of this motion, had brought that statute to parliament" should be punished "as traitors and criminals." Just which statute was meant is not evident, for in the strict sense, none such survives. The king's opponents in 1386 apparently mistook for a statute the entry on the *Rolls of Parliament* of Sir William Trussell's renunciation of allegiance to Edward II. Furthermore, at the parliament of 1386, Richard II's bellicose and ambitious uncle, Thomas, duke of Gloucester, had urged the king not to forget that the people, "by ancient statute and recent precedent," had a remedy for royal wrongs. All of this lends credence to the story that Gloucester and the lords appellant had, in December 1387, deposed Richard

⁸ S. B. Chrimes, "Richard II's Questions to the Judges, 1387," *Law Quarterly Review*, 72 (July 1956): 374. For studies of this deposition, see M. V. Clarke and V. H. Galbraith, "The Deposition of Richard II," *Bulletin of the John Rylands Library*, 14 (1930): 125-81; Gaillard Lapsley, "The Parliamentary Title of Henry IV," *English Historical Review*, 49 (1934): 423-49, 577-606, and "Richard II's 'Last Parliament,'" *English Historical Review*, 53 (1938): 53-78; H. G. Richardson, "Richard II's Last Parliament," *English Historical Review*, 52 (1937): 39-47; and "The Elections to the October Parliament of 1399," *Bulletin of the Institute of Historical Research*, 16 (1939): 137-43; Bertie Wilkinson, "The Deposition of Richard II and the Accession of Henry IV," *English Historical Review*, 54 (1939): 215-39, and *Constitutional History of Medieval England* (London, 1948), 2: 284-327; J. M. W. Bean, "Henry IV and the Percies," *History*, 44 (1959): 212-27; and, finally, Gerard Caspary, "The Deposition of Richard II and the Canon Law," *Proceedings of the Second International Congress of Medieval Canon Law*, Monumenta Iuris Canonici Subsidia, (Rome, 1966), 1: 189-201. For evidence of the distrust of Richard II, see [Jean Créton], *Histoire du Roy d'Angleterre, Richard [II]*, ed. and trans. John Webb, in *Archaeologia, or Miscellaneous Tracts Relating to Antiquity* (London, 1824), 20; esp. 137-41 where Créton, a supporter of Richard, reports that king's remarkably perfidious speech at Conway.

⁹ *Rotuli Parliamentorum*, ed. J. Strachey (London, 1767-77), 3: 224, 233. Hereafter cited as *RP*.

II and that he had remained “uncrowned” [*discoronatus*] for three days. Then after his opponents had removed Richard’s officers and councilors, they had “re-crowned” [*recoronant*] him king. Even Richard’s partisan, Jean Créton, described this parliament to that monarch as “your parliament, wherein you shall be highly crowned a sovereign king.”¹⁰ Whether the chroniclers were here retailing fact or fiction matters little, for their stories show that, whatever the case, some of Richard II’s subjects thought of Edward II’s deposition as a precedent and that circumstances might again rise that would remove the stigma of sacrilege from a king’s dethronement.

Nevertheless, Richard II’s deposition came only in 1399, after twelve years more of willful rule through his Cheshire archers, his Welsh pikemen, his over-eager bureaucrats, and sycophantic courtiers. He had given flesh and blood to his and his justices’ idea of what a king of England ought to be. The Lancastrians, whose *Record and Process of Richard II’s Renunciation and Deposition* denied that king’s rationale of his rule, no doubt exaggerated when they delineated the features of the bad king in their thirty-three articles of charges against Richard. They enriched their portrait with his personal defects and his professional faults. Not simply the conventional tyrant or despot of medieval political theology, he was a villain who embodied the vices and mistakes of all his predecessors on England’s throne. The offenses listed pertained to one or the other of the king’s two bodies, the body politic and the body natural. The “sentence” of deposition concluded that Richard personally, in his body natural, was “useless, unqualified, incompetent, and unworthy” for the “rule and governance” of Englishmen. Here the Lancastrians adopted, and much more explicitly than in 1327, the canonical doctrine of the *rex inutilis*—the useless, inadequate, inefficient, and inept ruler—a doctrine that distinguished between a king’s crimes and wrongdoings and his incompetence. The canonists held such a king “guilty of no sin except ignorance and inadequacy,” and back in 1245 Pope Innocent IV had pronounced King Sancho II of Portugal such a sovereign. Some canonists argued that a useless king should not be deposed, but that a coadjutor should do his work; when deposition seemed unavoidable, however, charges of dissoluteness were added. So the committee in 1399 charged Richard II with the crimes listed in the Decretals, to wit: “perjury, sacrilege, sodomy, plundering of subjects, reducing the people to servitude, insanity, and incapacity to rule”—“with all of which crimes,” Adam of Usk, a canon lawyer, chronicled, “King Richard was known to be tainted.”¹¹

Perjury was the count that the Lancastrian apologia brought most fre-

¹⁰ *RP*, 3: 233; McKisack, *Fourteenth Century*, 444; Chrimes and Brown, eds., *Select Documents*, 145; Créton, *Histoire*, 35. On one manuscript roll the renunciation is written between Edward III’s first and second statutes: H. G. Richardson and G. O. Sayles, *Rotuli Parliamentorum Angliae Hactenus Inediti* (London, 1935), 100.

¹¹ *RP*, 3: 422 (nos. 51, 52); Edward Peters, “*Rex Inutilis*: Sancho II of Portugal and Thirteenth-Century Deposition Theory,” *Studia Gratiana*, ed. Stephan Kuttner, IV, vol. 14 (1967): 300; Edward Maunde Thompson, ed. and trans., *Chronicon Adae de Usk, A.D. 1377–1421*, (2nd ed.; London, 1904), 29–30, 181; cf. *Corpus Juris Canonici, Liber Sextus Decretalium Domini Bonifacii Papae VIII*, Lib. II, Titulus XIV, sec. ii: *Papa Imperatorem deponere potest, ex causis legitimis*.

quently against the king, and ten of the thirty-three articles contain the word itself. John de Burgh's tract for priests defined perjury as confirming a lie under oath. Allegedly, Richard was guilty of this sin by breaking his promises to Henry, duke of Lancaster, about his exile and inheritance. The king had also violated his coronation oath when he broke laws, like the one limiting the sheriff's term of office or the antipapal statutes that he had promised to keep. Richard II had denied the liberties of the Church and taken money from the clergy, again contrary to his coronation oath. He had exiled Archbishop Arundel "without any reasonable or lawful cause, or any process of law, against the laws of the realm." After Richard had sworn on the cross of St. Thomas of Canterbury that Arundel would not lose his archbishopric, he had asked the pope to translate him. There were other frauds and deceits, like his promise to remit damages to the duke of Gloucester, whom he had murdered instead. The king had confiscated the lands of lords Warwick and Cobham, after their executions, "against justice, the laws of the realm, and his express oath." His opponents construed certain clauses in Richard's last testament as irrational and "repugnant to all right and reason." Furthermore, the king had made other erratic pronouncements: "that the life of any one of his lieges, and his lands, tenements, goods, and chattels are at his will without any [process of] forfeiture."¹² Richard had terrorized some men, like the justices who had answered the Ten Questions, by threats and fear of death. His troops raged through the countryside and committed robbery, rape, and murder, but the king refused to answer complaints or to do justice. He had deceived the people of seventeen counties; he had extorted money and had failed to pay his debts. Finally, he had ordered the *Rolls of Parliament* changed and records thereon deleted.

Beyond the seas, Richard II personally had made an impression that had damaged England's reputation "among strangers throughout the whole world." He had been "unstable and dissimulating," especially in writing to popes, kings, and lords; and "no living person knowing of his condition could or would trust him." Moreover, there were many who considered him "unfaithful and inconstant," and all this was "to the scandal of not only his person, but also of the whole kingdom."¹³ In short, the Lancastrian catalog of Richard II's personal defects depicted a mighty bad king.

More significant constitutionally were the things that Richard's body politic had done. It had acted "against the crown and royal dignity and against the statutes and liberties of the realm, . . . the laws of the land," and the laws of chivalry. Specifically, Richard had broken his coronation oath (recorded verbatim in the *Record and Process*), and he had violated c. 39 of Magna Carta. Some subjects who had spoken words in derogation of the king's person had been "taken and imprisoned and brought before the constable and marshal of England in military court." More offensive yet was Richard's request of the

¹² *RP*, 3: 418 (no. 21), 421 (no. 48), 420 (no. 43).

¹³ *Ibid.*, 420 (no. 42).

pope to confirm statutes enacted in the parliament of 1398 “although the crown of the kingdom of England and the rights of the same crown and the realm itself, have been from all past time free thereof.” Furthermore, Richard II had “expressly stated, with an austere and shameless face, that his laws were in his mouth and at times in his breast, and that he alone could change and enact laws of his kingdom.”¹⁴

Here the Lancastrians had cut to the root of the constitutional conflict between Richard II and his baronial opponents: Was the king of England to be the spokesman of the law, the law speaking, *lex loquens*? Or was the law of England to be what the king willed and said it was to be, *rex loquens*? In this way, then, two competing authorities, the king’s and parliament’s, and two contradictory doctrines of the source of English law elevated what was in origin a family feud and an interbaronial struggle for political power onto the plane of principle.

The committee of “doctors, bishops, and others,” such as Adam of Usk, had begun to draft the Lancastrian condemnation of Richard II only on September 21, after the king was safely in the Tower of London. Nine days later, September 30, those present in Westminster Hall used the authority of the estates of the realm and of the people to sanction Richard’s deposition. The day before, a commission of sixteen interviewed the king. Included were lords spiritual and temporal, the two chief justices, two doctors of law, two notaries, and others learned “in civil and canon law as well as the laws of the realm.” In a cheery tone, they reported back to the assembly the next day things both fanciful and fallacious—and a few authentic facts. The king had said “publicly with a happy countenance, that he was prepared to renounce and to cede” the throne to Henry “according to his promise”; and he had absolved all his liegemen from their oaths of fidelity and homage to him and “from every bond of allegiance and regality and lordship.” Richard had asked the archbishop of York and the bishop of Hereford to announce this “cession and renunciation to all the estates of the realm and . . . his will and intention to the people.” As a sign of his intent, Richard had taken off his gold signet ring and placed it on Duke Henry’s finger and asked him to show it to all the estates. In the event, though, this attempt to designate the succession purely on the basis of the king’s royal authority and “promise” was to prove insufficient.¹⁵

After the archbishop of York had described this scene in the Tower, he read

¹⁴ *Ibid.*, 419 (nos. 27, 29, 28, 33), 420 (nos. 43, 44), 421 (no. 47). For understanding Richard II’s misrule, see Caroline M. Barron, “The Tyranny of Richard II,” *Bulletin of the Institute of Historical Research*, 41 (1968): 1–18 and, good for specific details, though now dated, Reginald Rankin, *The Marquis of d’Argenson and Richard II* (London, 1901), 137–300. Much more sympathetic to Richard is Anthony Steel, *Richard II* (Cambridge, 1941).

¹⁵ *Chronicon Adae de Usk*, 29; *RP*, 3: 416 (nos. 10, 12, 13), 417 (no. 14). Although the insufficiency of Richard II’s authority to designate his successor (so different from that of William the Conqueror) appears largely the product of practical caution on the part of the Lancastrians, by 1460 the inability of a king to designate the succession was more clearly the consequence of constitutional as well as political considerations: see below, at notes 20–5. Yet questions relating to the succession were already beginning to exceed a purely royal authority; even Richard II found it desirable to seek parliamentary recognition of his own designated heir, Roger Mortimer: S. B. Chrimes, *Constitutional Ideas in the Fifteenth Century* (Cambridge, 1936), 24, n. 1.

the cession and renunciation, first in Latin, then in English. Next, the archbishop of Canterbury asked “the estates and the people” whether they wished to accept the king’s resignation “for their benefit and for the profit of the realm.” Thereupon, “the same estates and the people thinking . . . this would be very expedient . . . unanimously and harmoniously accepted the renunciation and cession.” This done, “all the estates” were asked “singly and in common” whether Richard’s “crimes and defaults were sufficient and notorious enough to depose the same king.” Once again, “all the estates consented unanimously.” Thereupon, “the estates and communities” appointed deputies “to depose King Richard of every royal dignity, majesty, and honor, in the place, name, and authority of all the aforesaid estates, as has been observed in similar cases by ancient custom of the said kingdom.”¹⁶

When the commissioners returned to the Tower on Wednesday, October 1, to depose Richard II formally, they again invoked the authority of the estates and of the people. To justify this dethronement, Chief Justice Thirning and his fellow procurators explained that “all the estates of this land” had “given them full authority and power and charged them for to say the words” that he would address to “Sir Richard, formerly king of England.” He told how “all the estates and all the people,” gathered at Westminster on Tuesday, had thought that the “articles of defaults in your governance” were “reasonable and cause for to depose you.” The commissioners, “representing the peers and nobles, spiritual and temporal, of the realm of England and the communities of the realm and all the estates of the realm,” had delivered “in open court” their “definitive sentence.” Then the chief justice concluded: “We, procurators to all these estates and people aforesaid, . . . by their authority given us, and in their name, yield you up for all the estates and people aforesaid, homage liege and fealty and all the allegiance and other bonds, charges, and service that belong thereto.” Thus in law the authority of the people and of the estates of the realm—that is, the *kingdom*’s authority, not parliament’s—triumphed over Richard II, his will and his willfulness, and over God’s anointed.¹⁷

This apologia of the Lancastrians states or implies that the kingdom, the estates of the realm, and the people speaking through their self-designated proxies had a right to depose. What underlies all the charges against Richard II (and the earlier ones against Edward II as well) is a tacit assumption that the kingdom has authority, perhaps supreme, one sanctioned by those four potent little words, *vox populi, vox Dei*.¹⁸ In theory, then, the people’s voice expressed the people’s choice—and God’s.

¹⁶ *RP*, 3: 417 (nos. 15, 16), 422 (no. 51).

¹⁷ *Ibid.*, 424 (nos. 58, 59, 60), 422 (no. 50). The phrase, “by authority of parliament,” first appeared only in 1398 when the Shrewsbury parliament authorized the appointment of a committee to deal with petitions. Then in Henry IV’s parliament of 1399, statutes were said to bind until they were revoked “by authority of another parliament.” Chrimes, *Constitutional Ideas*, 138, n. 4, quoting from *RP*, 3: 419.

¹⁸ The right to depose in 1399 seems to have derived from the authority of the estates of the realm and/or the people, perhaps the kingdom as an abstract concept, but not from parliament’s authority. By 1484, however, the authority of the estates was enhanced by their being “in” parliament, whose authority seems by then to have acquired a higher degree. The purpose of using the concept of estates in 1399 seems to have

But the reasons given to justify a king's deposition were not the same as those required to authorize his successor's right to rule. In 1399, however, the sources of authority were much the same, and Henry IV succeeded Richard II as God's choice, as the people's, and a bit less mystically as that of the estates of the realm. The representatives who spoke for these estates were for practical reasons the high officers of state—councilors, justices, and men of law—reinforced by those lords spiritual and temporal and the knights and burgesses who happened to be at Westminster because of Richard II's writs of summons to the king's parliament.

Before this assembly, the duke of Lancaster arose and spoke, in English. Henry claimed as his right "this realm of England and the crown with all the members and appurtenances . . . [i]n the name of the Father, Son, and Holy Ghost." He did so as "descended by right line of the blood" from Henry III and "through that right that God of His Grace hath sent me with the help of my kin and of my friends to recover it." When Henry had finished, "the lords spiritual and temporal and all the estates there present, singly and in common, were asked how they felt about this claim and vindication." Thereupon, the "same estates with all the people without any difficulty or delay unanimously consented . . . that the aforesaid duke should reign over them." When Henry had thanked "God and you, spiritual and temporal, and all the estates of the land," a parliament (a thing hitherto not mentioned) was announced for the next Monday, October 6, and the coronation a week later on the Feast of St. Edward the Confessor. Henry IV himself acknowledged the authority of the estates when he described his title to the Doge of Venice "as by right of birth and by the unanimous consent of the lords and commons, we possess the royal scepter." The Percies were to tell Henry IV in 1403 that "you crowned yourself king," but others, less doubtful, considered him a king "chosen [*electus*] by God," who had sanctioned his right to rule.¹⁹

The next critical date in the transit of ideas that shaped the right to rule was October 25, 1460. On that day King Henry VI contracted, and the lords of parliament agreed, that Richard, duke of York, should be his lawful heir and legitimate successor to the crown. Henry's failure to run an efficient government, to win the Hundred Years War, and to maintain law and order within the realm had quickened men's curiosity about his authority to govern. After the lords of parliament had adjudged York's claim to the crown superior to Henry VI's, the king "condescended to [an] accord to be made between him

been to demonstrate that those assembled in Westminster Hall were present as representatives of the kingdom—which parliament, a century later, was to become. In 1399 parliament was more the king's high court than the legislative or legislature—words that first appeared in 1689 and 1676 (see *O.E.D.*, *sub verba*). In 1399 "the estates and the people" seem to have been a more comprehensive body, representative of the whole realm, as against the king's high court and his great council whose original functions were to give the king counsel and consent. By 1484, though, parliament had become such a body, so that it could, and did, comprehend the three estates, if not the people. See Charles T. Wood, "The Deposition of Edward V," *Traditio*, 31 (1975): 277–83.

¹⁹ *RP*, 3: 422–423 (nos. 53, 54, 56, 57); *Calendar of State Papers, Venetian*, 1: 39 (no 131); Bertie Wilkinson, *Constitutional History of England in the Fifteenth Century (1399–1485)* (New York, 1964), 45, quoting from Hardyng's *Chronicle*, ed. H. Ellis (London, 1812), 352–53; H. T. Riley, ed., *Johannis de Trokelowe, et Henrici de Blaneфорde, Monachorum S. Albani, necnon Quorundam anonymorum Chronica et Annales* (London, 1866), 300.

and the said duke and to be authorized by the authority of this present parliament."²⁰ Here, it is submitted, was the crucial point of recognition that guaranteed for parliament and its authority a future, and for the monarchy a limitation that would put the kingship, and the king's servants too, under law.

Constitutionalism runs through the whole rationale that Richard, duke of York, used in his bargaining for the crown. The notion that the Yorkists pursued a doctrine of legitimacy seems much too modern a concept, in fact post-Metternichian, for the undefined and ambiguous rules of inheritance to the English kingship in the fifteenth century. After all, three separate Lancastrians had enjoyed the crown for sixty years, and Henry VI, unlike Richard II, had a direct blood heir. To counter this prescriptive right, the Yorkist lawyers in 1460 argued not legitimacy, but only that the descendants of Edward III's third son, Lionel, duke of Clarence, should precede those of the fourth, John of Gaunt; and, for good measure, that York also descended from the fifth son, Edmund, duke of York. Nevertheless, the elder line came down through two women, Philippa and Anne, and the legality of inheritance through one female, let alone two, had yet to be confirmed. Except for this possible bar feminine, though, the sanction of hereditary right, with its divine overtones, put Richard, duke of York, and his sons in a strong position, provided they could find an authority high enough to recognize the legal validity of their claim. In the event, Henry VI, of "his assent and agreement, of his free will and liberty," recognized the lords' conclusion, "authorized by the authority of this present parliament," and agreed that Richard of York was his heir apparent, thereby disheriting his seven-year-old son, Prince Edward of Lancaster.²¹

This accord or contract first recited York's genealogy. Then it explained that the duke, "for the weal, rest, and prosperity of the land," had "agreed and consented" that Henry VI, who had possessed England and the kingship so long, should be "reputed and taken" to be king "during his life natural." York and his two older sons, Edward, earl of March, and Edmund, earl of Rutland, were to swear oaths not to do anything "to the abridgement of the natural life of King Harry VI." In return, York was to be "called and reputed from henceforth, very and rightful heir to the crowns, royal estate, dignity, and lordship abovesaid." By this agreement, the duke was to receive, "by authority of this present parliament," lands, castles, and manors to the yearly value of 10,000 marks. The accord also declared York's person inviolable, and

²⁰ *RP*, 5: 378 (no. 18). For background, see J. R. Lander, "Henry VI and the Duke of York's Second Protectorate 1455 to 1456," *Bulletin of the John Rylands Library*, 43 (1961): 46-69; also Charles Ross, *Edward IV* (London and Berkeley, 1974), 12-29.

²¹ On the weak hereditary principle in England and its relationship to depositions, see Charles T. Wood, "Queens, Queens, and Kingship: An Inquiry into Theories of Royal Legitimacy in Late Medieval England and France," *Order and Innovation in the Middle Ages*, ed. W. C. Jordan, C. B. McNab, and T. F. Ruiz (Princeton, 1976), 385-400, 562-6. *RP*, 5: 378 (no. 18), 379 (no. 27). B. P. Wolfe has made out a strong case that Henry VI was more king than saint, but not successful as either. He maintains that Henry VI conducted an aggressive government under a "personal rule" that engendered "dissensions and mutual recriminations, accusations of deceit and bad faith"—all characteristics of the Bad King. B. P. Wolfe, "The Personal Rule of Henry VI," *Fifteenth-Century England, 1399-1509*, ed. S. B. Chrimes, C. D. Ross, and R. A. Griffiths (New York, 1972), 42.

anyone who plotted or compassed his death was to be judged guilty of treason. Finally, the lords of parliament were to take oaths to accept, worship, and repute Richard and his heirs “as above is rehearsed”; and the duke and the two sons were, in turn, to swear oaths “to help, aid, and defend the said lords.” All that remained to complete these arrangements was for God to intervene and terminate Henry VI’s “life natural.”²² Then the crown would pass to a new king, one for the first time genuinely and legally made-in-parliament—and under the law of contract.

Before the lords of parliament and the men of law reached this conclusion, they had searched their souls and the records. The duke of York had submitted his claim to the crown on October 16, 1460, in parliament. Since it was one on which Henry VI alone clearly lacked the practical and legal authority to rule, king though he might be, the lords called “the king’s justices into the parliament chamber” on the 18th “to have their advice and counsel.” In the king’s name they ordered them “to search and find all such objections” against York’s claim and to fortify “the king’s right.” These judges, lacking the stuff supreme courts are made of, ducked the great constitutional issue with its risk of treason: to define, and hence to limit, the English kingship. They refused to determine the rightful inheritor of the crown, to perform a judicial review of regal rights, or, in short, to make public law. For the king’s justices to have done so would have been to have placed themselves and their successors above their king, thus putting the crown under the judiciary. Instead, the judges reported back to the lords on Monday, October 20, that they would give no opinion because “they were the king’s justices, and have to determine such matters as come before them in the law, between party and party, . . . and since this matter was between the king and the said duke of York as two parties,” they would not give Henry VI counsel. Moreover, and here the constitutional issue came in, “the matter was so high and touched the king’s high estate and regalie, which is above the law and passed their learning, wherefore they durst not enter into any communication thereof, for it pertained to the lords of the king’s blood, and th’apparage of this his land, to have communication and meddle in such matters.”²³ Not yet were the justices proper lions under the king’s throne, or if they were, they did not care or dare to roar in Henry VI’s behalf.

Instead, the lords of parliament, irked, sent for the serjeants-at-law and the king’s attorney and ordered them in the king’s name to “search and seek” for all things that would avail the king and defeat the duke’s title. Two days later, Wednesday, October 22, these men of law stood on Monday’s precedent and declined to communicate: since “the said matter was so high that it passed the learning of the justices, it must needs exceed their learning.” Next, the chancellor, George Neville, tried to compel the serjeants to give advice or counsel because “they had their fees and wages” from the king. To this the serjeants made a remarkable statement about the king’s authority: “They

²² *RP*, 5: 378–9 (nos. 20, 21, 22, 23, 25, 26).

²³ *Ibid.*, 375 (no. 10), 376 (no. 12).

were the king's counselors in the law, in such things as were under his authority, . . . but this matter was above his authority, wherein they might not meddle."²⁴

The king's own authority, that of the king's justices, and the authority of the law and its serjeants, each had failed to enable the lords to decide who had the right to rule and be king. They were forced to do it themselves. They had a free debate and agreed "that every lord should have his freedom to say what he would say without any reporting or maugre [ill-will] to be had for his saying." Thereupon they negotiated the compromise between Henry VI and Richard, Duke of York. The agreement, comparable to the treaty between King Stephen and Henry II, was an "act of accord, by the king and the three estates, in this present parliament assembled."²⁵ In politics, the advice and assent of the lords of parliament and of the commons validated the agreement; yet in law it carried the sanction of an authority only vaguely hinted at in 1399, but now made explicit: that of the three estates of the realm. The compromise was illogical and contradicted theory and theology. And yet it was so sensible. As the product of expediency, it offered a political solution to a legal problem. After all, York had arrived with 300 armed men, and the sanction of force and arms was never far in the background. The lords of parliament were too evenly divided in their partisanship to stop for finespun theorizing; yet what they said "as of record" on that October day about king and parliament was to be long remembered.

The very use of parliament and of the phrase, "by authority of parliament," to resolve this crisis gave precedent and principle for a parliamentary monarchy. Henry VI's parliament of 1460 had repealed the statute of 1406 that had declared the sons of Henry IV and their heirs successors to the throne. The lords of parliament in 1460 had declared such acts to be of "more authority than any chronicle, and also of authority to defeat any manner [of] title made to any person." And yet this same parliament, by repealing the statute of 1406, switched the order of succession to the Yorkist line, and so, by implication, indicated that a parliament might not be bound by its predecessors' acts. The duke of York, himself, agreed that after Henry VI's death his title would be one "entailed by authority of parliament," and in so doing he set a precedent for Henry VIII's three Succession Acts that determined the sequence of his assorted heirs.²⁶

Had Henry VI but died immediately, this agreement would have become

²⁴ *Ibid.*, 376 (no. 12).

²⁵ *Ibid.*, 376 (no. 12), 379-80 (nos. 29, 27).

²⁶ *Statutes of the Realm*, ed. A. Luders *et al.* (London, 1810-28), 2: 380; *RP*, 5: 377 (no. 15), 379 (no. 28); St. Thomas More, *The History of King Richard III*, ed. R. S. Sylvester (New Haven and London, 1963), 67. It should be noted, too, that over a year previous to the October parliament of 1460 the earl of Warwick had refused to yield up the captaincy of Calais in response to Henry VI's personal authority under the privy seal. The earl argued that "forasmuch as he was made by authority of parliament, he would not obey the privy seal." Wilkinson, *Fifteenth Century*, 287, quoting from Davies' *English Chronicle*. It would appear, then, that 1460 was not the first year in which the authority of parliament was employed to justify a position hostile to the wishes of the then-reigning king, thus implying that parliament's authority might be superior to that of the king.

operative. Unfortunately, God's will was otherwise, and events destroyed the logic of the settlement. The wager of battle at Wakefield, December 30, 1460, went against the duke of York; he was killed, and Henry VI survived. Nevertheless, he too was soon defeated, but not killed, at Towton in Yorkshire at the end of March 1461. The contractual agreement still held, for it applied equally to York's heirs. Had Henry VI been slain in battle, then Edward IV would have succeeded under the terms of the contract of 1460, a written one signed and sworn to and sanctioned "by authority of parliament." This would have made Edward IV a purely parliamentary sovereign. But unhappily for England's public law and contractual monarchy, God let Henry VI live on and flee with Queen Margaret to Scotland, thence to France where they maintained a government-in-exile.

Real life being different from theory, Edward IV thus won his crown by military victory, not in parliament. The Lord God of Hosts had shown him His favor and had expressed His choice of king. So with the victor in possession of the throne, God's servants confirmed men's maneuverings through episcopal connivance and ecclesiastical ritual on June 28 at Edward's coronation. Though the new king had proclaimed his accession on March 4, 1461, he did not call a parliament to meet until November 4. On that parliament's *Rolls* "of record" are explanations of Edward IV's title, of his right to rule, and of his authority to govern. The sanctions that the Yorkists invoked derived from God, from the law in its various emanations, from the realm, and from parliament. The kingdom's authority, expressed through the three estates of the realm, was in the process, not yet complete, of being transformed into "the authority of parliament." Edward, with "the advice and assent of the lords spiritual and temporal and of the commons," declared himself king "by God's law" of inheritance, with parliament inscribing his royal pedigree on its *Rolls*.²⁷ The commons' petition thanked God for His decision and Edward for his willingness to assume the burdens of the English crown.

Since Henry VI lived on, King Edward IV could not make his case on the basis of the agreement of 1460 between King Henry and his father. So Edward cleared the way for his inheritance by an act of parliament that declared the three Henrys to have been kings *de facto* but not *de jure* because of Henry IV's "intrusion and usurpation." The Yorkists did not denigrate Henry VI personally as Henry IV had done to Richard II. Instead, the commons condemned the "extortion, murder, rape, effusion of innocent blood, riot, and unrighteousness" that had flourished without punishment during Henry VI's "usurped reign." Their petition contrasted Henry VI's "abusion of the laws" with the benefits of a good king's "plenty, peace, justice, good governance, policy, and virtuous conversation." To achieve these ends, the good king should rule by an authority external to his person. Moreover, such a king was expected to be responsible to something more than to God and to his own conscience—perhaps to the people, certainly to the law. Edward IV's right to rule was said

²⁷ *RP*, 5: 464 (no. 10).

to be “by God’s law, man’s law, and the law of nature.” Above all, the good king and his ministers would govern through procedures duly recognized as lawful—what Henry IV’s council had described in 1408 as “due process of the law.” Finally, this petition in 1461 endorsed the agreement of 1460 with the duke of York and declared Edward “in right, from the death of . . . his father, very just king of the said realm of England.”²⁸ So much for God and the law.

If those who drafted Edward IV’s apologia overlooked the people’s authority, contemporary chroniclers did not. Several tell how the people enjoyed a vociferous role in Edward’s usurpation when they bestowed, in March 1461, their authority on his succession. On three occasions, when asked if they wanted Edward, the people shouted “Yea”; but the chroniclers do not mention any election by the estates of the realm. Edward IV and his sponsors held meetings in London, one on March 1 in St. John’s field. There “the people” and an army—some “3,000 or 4,000 men”—had gathered, and “certain articles and points were read to them, in which King Henry VI had offended against the realm.” First, the people were asked whether the said Henry was still fit to reign, and they cried “Nay.” Then they were asked if they would have Edward, Earl of March, to be their king, and they cried “Yea.” Thereupon certain captains and many people went to Baynard’s Castle, and there they told the Earl of March “that the people had chosen him to be king; and he thanked them . . . and granted that he would take it upon him.” Three days later, on March 4, the people were invited to meet Edward at St. Paul’s at nine o’clock, and there the bishop of Exeter gave a sermon and “set forth the right and title of the Earl of March to the crown; and he demanded of the people if they would have the earl to be their king, as due to his right, and they cried ‘Yea.’” At St. Paul’s, Edward was “elected and admitted for king.”

All the people then went with him to Westminster Hall, and there Edward “was sworn before the archbishop of Canterbury, the chancellor of England, and the lords, that he would truly and justly keep the realm and maintain its laws, as a true and just king. They put on him the king’s robes and the cap of estate; and he went and sat on the throne as king. It was demanded of the people if they would have him to be their king, and would maintain, support, and obey him as true king; and the people cried ‘Yea.’”²⁹

²⁸ *Ibid.*, 464 (no. 10), 463 (no. 7); Harris Nicolas, ed., *Proceedings and Ordinances of the Privy Council in England* (London, 1834–37), 1: 309. To do all this was necessary because Edward IV, like his father, had sworn to keep the terms of the contract of 1460, which allowed Henry VI to reign for his natural life. In order to circumvent the “convention, concord, and agreement between Henry and the duke,” Edward IV’s parliament had “declared and judged, by the said advice, assent, and authority,” that Henry VI had, “against good faith, truth, conscience, and his honor brake the said convention and concord, and departed therefrom of willful malice.” Therefore, the petition concluded, “the breach thereof, on his part, discharged” Edward from “the keeping thereof of any particle or point after the said breach.” *RP*, 5: 466 (no. 14). The extent to which all kings were responsible to authority external to their persons, especially the law, was also emphasized by Edward’s Lancastrian opponent, Sir John Fortescue: cf. David S. Berkowitz, “Reason of State in England and the Petition of Right, 1603–1629,” *Staatsräson: Studien zur Geschichte eines politischen Begriffs*, ed. Roman Schnur (Berlin, 1975), 168–72. Strikingly, though, Berkowitz demonstrates (p. 170) that Fortescue allows for and justifies “royal power” as “derived not from the prince’s authority but solely from the law of nature.”

²⁹ Wilkinson, *Fifteenth Century*, 173–4.

The notion that the people chose the king was current in 1461. The duke of Milan's emissary to France reported to Francesco Sforza "how the people of London, the leaders of the people of the island, together with some other lords, full of indignation had created a new king. . . . [H]e was chosen, so they say on all sides, as the new king by the princes and people at London." Nevertheless, the crucial decision was made at a council of lords and bishops in the smoke-filled rooms of Baynard's Castle "where they agreed and concluded that Edward himself, the duke of York, should then be king of England." After this, Edward IV received the vulgar sanction of the people's will (or willingness), and presumably their authority. Thus God and the people, the lords spiritual and temporal, and Edward IV's armed retainers too, participated in creating a new king of England according to the laws of God, man, and nature. Eight months later, in November 1461, the action was confirmed "by the authority of parliament."³⁰

When Edward IV's brother, Richard, duke of Gloucester, intercepted the crown as it was descending upon his twelve-year-old nephew, Edward V, in June 1483, he followed a scenario drawn from the earlier usurpations.³¹ Edward IV's last wishes had made Richard Protector of the realm with the authority to govern. But becoming afraid of losing his power to Elizabeth, the queen-mother, and her Woodville faction, Gloucester aimed to get possession of the young king and his brother. Edward V's coronation had first been scheduled for May 4 by the Woodvilles, but the Protector then postponed it until June 22. He gained control over the king's person on April 30, but did not get his brother into his hands until June 16. Perhaps on the 17th, certainly by June 22 when the "process of deposition" had begun, Richard decided to usurp the throne in order to preserve the power of governance that the Protectorship allowed.³² So the boys went to the Tower; they were never seen again outside its walls. By December 1 rumor of their deaths had reached Vienne on the Rhone.

Richard III's motivation came more from fear (see his portraits) than from villainy. As long as another person, even a boy, was the true and living sovereign, one whose will a rival counselor might declare, Richard lacked the right to give forevermore "the last and highest commandment." So to clear obstructionists from his way to the throne, in mid-June Richard had the bishops, Rotherham and Morton, dismissed from the council and imprisoned in Wales; at the same time, his recent aide and ally Lord Hastings, Edward IV's chamberlain and sworn man, was murdered; and on June 25, the queen-mother's brother and son, Earl Rivers and Lord Richard Grey, were executed

³⁰ *Calendar of State Papers and Manuscripts Existing in the Archives and Collections of Milan*, ed. Allen B. Hinds (London, 1912), 58 (no. 76); Wilkinson, *Fifteenth Century*, 176.

³¹ See C. A. J. Armstrong, "The Inauguration Ceremonies of the Yorkist Kings, and Their Title to the Throne," *Transactions of the Royal Historical Society*, 4th series, 30 (1948): 51-73 for a comparison of Richard III to Edward IV; Wood, "Edward V," 277-82 for one to Henry IV. See also J. R. Lander, "Marriage and Politics in the Fifteenth Century: The Nevilles and the Wydevilles," *Bulletin of the Institute of Historical Research*, 36 (1963): 119-152.

³² E. F. Jacob, *The Fifteenth Century, 1399-1485* (Oxford, 1961), 614. For the circumstances that provoked Richard, duke of Gloucester, to usurp the crown, see Wood, "Edward V," 248-68.

at Pontefract as “conspirators of Richard’s death.”³³ Back in London, Richard’s agents put on a travesty concocted from accounts of the three previous depositions. Dr. Shaw, the mayor’s brother, acted the role played by bishops in 1327 and 1461. He preached on Sunday, June 22, not in St. Paul’s Cathedral, but outside at Paul’s Cross. His text came from Wisdom iv: 3: “Bastard slips shall not take deep root.” Thereupon, he proceeded to bastardize Edward IV and his sons and daughters; and he declared Richard, “who altogether resembled his father,” to be “the legitimate successor.” Unlike the meeting held in 1461 at St. Paul’s where the people gave their “yeas” for Edward IV, at Dr. Shaw’s meeting only “a small number of listeners cried ‘yea!’ . . . more for fear than for love.” Thomas More later explained this failure as the result of bad timing, for Richard was scheduled to “have come in among the people to the sermonward, to the end that those words meeting with his presence, might have been taken among the hearers, as though the Holy Ghost had put them in the preacher’s mouth, and should have moved the people even there, to cry King Richard, King Richard, that it might have been after said, that he was specifically chosen by God and in manner by miracle.” Unfortunately, Dr. Shaw spoke too fast—or Richard tarried too long and missed his cue—for the doctor had “entered into other matters ere the Protector came”; and then, flustered by the Protector’s arrival, he repeated Richard’s claim “out of all order, and out of all frame.”³⁴

This histrionic fiasco did not, however, deter the conspirators. On Tuesday, June 24, the duke of Buckingham, Richard’s ally, stage manager, and rhetorician (only to become his rebellious subject and rival in October), orated at the Guildhall. Those present “marvelled and said that never before that day had they heard any man, learned or unlearned, make such an exposition or oration as that.” The next day, the 25th, Buckingham led a few noblemen, knights, and gentlemen, along with the mayor and aldermen of London, to Baynard’s Castle, the home of Richard’s mother. There Richard, like Edward IV before him, awaited the people’s invitation to be king. Buckingham, on behalf of his companions, “made humble petition” that Richard take “upon him the crown and governance of this realm.” On Thursday, June 26, Richard rode from Baynard’s Castle to Westminster Hall where he received a bill of petition and “was set in the king’s throne or place where all kings first take possession.” Seated on the king’s bench in the royal chair, Richard acted out the scene from Edward IV’s script:

[H]e called before him the judges, commanding them straightly, justly, and duly to administer his law without delay or favor. . . . [H]e then proceeded into the Abbey. At the church door, he was met by a procession, and the abbot or his deputy delivered to him the scepter of St. Edward. He then proceeded into the shrine and there made

³³ Dominicus Mancinus, *The Usurpation of Richard the Third* (*Dominicus Mancinus ad Angelum Cantonem de Occupatione Regni Anglie per Riccardum Tercium Libellus*), ed. and trans. C. A. J. Armstrong (Oxford, 1936), 149 and n. 72, 152 and n. 82, 117 and n. 90.

³⁴ Wilkinson, *Fifteenth Century*, 189–90; James H. Ramsay, *Lancaster and York* (Oxford, 1892), 2: 488; More, *History*, 68.

offerings. He was next led into the choir and sat there whilst a *Te Deum* was beautifully sung by the monks. When these ceremonies were finished, he returned into the king's palace and was there lodged.³⁵

On July 6, a reluctant archbishop of Canterbury, Thomas Bourchier, anointed Richard III and crowned him king of England. Even without the moral opprobrium of the princes' murder in the Tower (Edward V may have died a natural death from osteomyelitis), Richard III went to his coronation with lies on his lips and blood on his hands.

The ways of Providence in 1483 may have mystified some men present at Richard's coronation, but more coolheaded prelates and legists took this opportunity to clarify the coronation oath. First of all, they wrote it in English, and the copy now at Lambeth Palace makes it quite certain which laws King Richard III was to "grant and promise" to keep: "Do you grant the rightful laws and customs to be held and promise you after your strength and power such laws as to the worship of God shall be chosen by your people . . . ?"³⁶ A king who made this promise, under oath, was certainly under the law—and virtually alongside his people.

Unfortunately, though, many Englishmen (including Richard, duke of Gloucester) had sworn oaths of fealty and had done homage to Edward V. The Protector himself had received the oaths of the mayor and notables of London. Richard III's lawyers were able to fix that. On June 28, they drew up "articles of instruction" to invalidate these premature oaths. If the mistake was easily pointed out and as easily rectified, credibility was harder to establish. Many "great estates and personages, being ignorant" of Richard III's title, had taken the oath of allegiance to Edward V, but "now every good true Englishman is bound upon knowledge had of the said very true title to depart from the first oath so ignorantly given . . . , and thereupon to make his oath of new and owe his service and fidelity to him [Richard] that good law, reason, and the concord assent of the lords and commons of the realm have ordained to reign upon the people. . . ." The coronation ritual, too, recognized Richard III's election by the three estates. When the archbishop of Canterbury presented "the king to the people" in Westminster Abbey, he designated him as "inheritor by the laws of God and man to the crown and royal dignity of England . . . elect, chosen, and required by all three estates of the same land to take upon him the said crown and royal dignity." Note, though, that in 1483 the word "elect" meant to choose, not to ballot, as it still did in 1526 when thirty of Henry VIII's great horses were to "be elected and chosen out of the best" for the royal stables.³⁷

³⁵ Wilkinson, *Fifteenth Century*, 190; More, *History*, 77–8.

³⁶ Chrimes and Brown, 355. In view of Wood's argument ("Edward V," 283–5) that Richard III's act of succession had the effect of extending the authority of parliament to spiritual affairs, it is significant that his coronation oath made him "grant and promise . . . such laws as to the worship of God shall be chosen by your people." Given the changes in authority then taking place, this development should scarcely be surprising.

³⁷ Chrimes and Brown, 353–4; *Rutland Papers*, ed. William Jerdon (London, 1842), 13; *A Collection of Ordinances and Regulations for . . . the Royal Household*, Society of Antiquaries (London, 1790), 160 (cap. 76); see also p. 147 of this last work where yeoman ushers were "to be taken and elected."

Far more significant than what actually happened in 1483 (for Richard's reign and life proved short) were the fabulous legal fictions in the people's petition, a purported version of which was entered on the *Roll of Parliament* in January 1484. This enrollment made its contents not only true and right in law, but operative in politics. Most notable were the constitutional fiction (revived in 1689) of an election by the three estates of the realm and their identification with parliament. The parliamentary version of the petition based Richard's right to rule upon three sanctions: "lawful election," "true inheritance," and "God's grace." There had been no formal wager of battle (though force had been used and blood shed) to enable God to make evident His choice or to admit justification by conquest, but the petitioners presumed to know His will. In the good old days when "our Lord God was dreaded, loved, and honored," England's rulers had sought "the common and politic weal of the land." The nation then had enjoyed "great prosperity," for England's kings had "followed the advice and counsel" of persons with "policy and experience, dreading God." In contrast, Edward IV in his "rule and governance of this land" had "followed the counsel of persons insolent, vicious, and of inordinate avarice, despising the counsel of good, virtuous, and prudent persons." Therefore, "felicity was turned into misery, and prosperity into adversity, and the order of policy, and of the law of God and Man confounded."³⁸

Proof that God willed Richard's inheritance to the crown to be the "true" one was the allegation that Edward IV had "stood married and troth plight" to Eleanor Butler by "a precontract of matrimony." This technical point of canon law permitted Richard to deduce that Edward IV had lived "sinfully and damnably in adultery" with Elizabeth, his queen. Moreover, the "pretensed marriage" had been made by sorcery and witchcraft and "without the knowing and assent of the lords of this land." This supposed adultery was, of course, "against the law of God and of His Church" and "the laudable custom of the Church of England." Finally, it perverted "all politic rule" and "the laws of nature and of England." Dr. Shaw had bastardized Edward IV himself, alleging a different paternity, but someone had the delicacy to delete this charge and so save the duchess of York from this indecency. Richard's other older brother, George, duke of Clarence (d. 1478) had left issue, but they were barred from claiming the crown "by an act made in the same parliament" that had attainted Clarence of high treason. Thus several kinds of law—divine and natural, canon, common, and statute—cleared the way for Richard to become the true heir to the English crown. Finally, the three estates prayed that "our Lord God, King of all Kings, . . . lighten your soul and grant you grace . . . so that after great clouds, troubles, storms, and tempests, the Son [sun?] of justice and of grace may shine upon us, to the comfort and gladness of all true Englishmen."³⁹

A nascent nationalism also supported Richard's succession, for he had been "born within this land." Edward IV was born at Rouen, and so Richard

³⁸ *RP*, 6: 240, 241.

³⁹ *Ibid.*, 241.

would be "more naturally inclined to the prosperity and common weal of the same." His birth was excellent, for he was "descended of the three most royal houses in Christendom, that is to say, England, France, and Hispanie." In addition to all this, the three estates, according to their petition, reinforced Richard's right to rule with their authority expressed through their election, one that Thomas More later called a "mock election." Yet More himself had King Richard III declare to the estates that to his title as "right heir . . . is now joined your election, the nobles and commons of this realm, which we of all titles possible take for most effectual."⁴⁰

No king of England did so much—on paper—as did Richard III to raise parliament's position in the frame of government. He exalted its authority in theory—and theory, Maitland remarked, "is more permanent than practice." The parliament of 1484 explained "how that the court of parliament is of such authority, and the people of this land of such nature and disposition, as experience teacheth, that manifestation and declaration of any truth or right, made by the three estates of this realm assembled in parliament, and by authority of the same, maketh, before all other things, most faith and certainty; and quieting men's minds, removeth the occasion of all doubts and seditious language." Such a declaration put parliament well on the road toward supremacy or sovereignty, for by implication it imposed upon Richard III a responsibility to that high court as his Maker. Evidence for this view dates from before the usurpation. The dying Edward IV had named his brother Gloucester to the Protectorship in early April, but Richard had found in practice that even this appointment needed approval by the council on May 10 before it carried legal weight. Moreover, this recognition, too, was soon regarded as insufficient. When Edward V's chancellor, Bishop John Russell, drafted a sermon with which to open parliament on June 25 (a meeting that Richard's *coup* prevented), he firmly asserted that the duke's "power and authority" as Protector had still "to be assented [to] and established by the authority of this high court." The ensuing summer and autumn were filled with signs of opposition to Richard's rule, like Buckingham's rebellion and Henry Tudor's first abortive invasion. Small wonder, then, that the parliament of January 1484 should have recorded that doubts and ambiguities had arisen in some men's minds about the validity of the new king's title, because "neither the said three estates, neither the said persons which in their name presented and delivered . . . the said roll [asking Richard to assume the crown] . . . were assembled in form of parliament." Or that this parliament should have ordained and established that "the tenor of the said roll . . . delivered to . . . the king in the name and on the behalf of the said three estates out of parliament, now by the same three estates assembled in this present parliament [1484], and by authority of the same, be ratified, enrolled, recorded, approved, and authorized."⁴¹

⁴⁰ *Ibid.*, 240, 241; More, *History*, 82, 80.

⁴¹ F. W. Maitland, *Constitutional History of England* (Cambridge, 1946), 197; Chrimes, *Constitutional Ideas*, 178; *RP*, 6: 240-1; Wood, "Edward V," 249, 251, 255-7.

The *Roll of Parliament* further stated that everything in the petition was to “be of like effect, virtue, and force, as if all the same things had been so said . . . in a full parliament and by authority of the same accepted and approved.” Here the potent little words, “as if,” indicate clearly that things done “in a full parliament,” whether by or for the three estates, carried the highest possible earthly authority. Thus parliament’s action in 1484 gave legality, if not legitimacy, to Richard III’s “lawful election” by the three estates back in June 1483. At that time, the estates had “chosen in all that in us is, and by this our writing choose you, high and mighty prince, into our king and sovereign lord. . . . We humbly desire, pray, and require your said noble grace, that, according to this election of us the three estates of this land, as by your true inheritance, you will accept and take upon you the said crown and royal dignity.” In return for Richard’s acceptance of the crown, the three estates made a promise—or contract—with the new king: “We promise to serve and assist your highness, as true and faithful subjects and liegemen, and to live and die with you in this matter, and every other just quarrel.”⁴² Thus did the subject’s oath of allegiance join with the king’s coronation oath to give the English monarchy its contractual nature.

Under Richard III, then, the three estates of the realm spoke for the kingdom. In 1483 they had made their pronouncements “out of” parliament; but when a continuing opposition to the regime demonstrated that this sanction was insufficient, the estates were compelled to repeat their declaration “in” parliament in January 1484 to give it both practical effect and legality. If what the three estates had said in June continued to be taken for legal truth, this was only because parliament in January affirmed it true. The legal fiction used in June was now to be accepted “as if” declared “in a full parliament and by authority of the same.” Thus in law the idea of the three estates proved convenient and useful for Richard III. It was an idea, however, that his successor, Henry VII, tried initially to avoid, for its clear statement of “election” implied a limitation on the kingship. The fiction of the three estates put upon the right to rule a restriction scarcely compatible with Henry Tudor’s exalted regal intentions.⁴³ When he arrived on Bosworth Field in August 1485 to depose Richard III, he found that the king had, quite literally, hung his crown upon a hawthorn limb as the prize for the victor. Nevertheless, Henry VII’s victory alone did not seem to provide him sufficient title.

The battle won, King Henry entered London on September 3 with the crown, that circlet of gold and precious stones, firmly in his possession. His formal coronation took place on October 30, and he met his first parliament on November 7, 1485. The first Tudor king may or may not have let the

⁴² *RP*, 6: 240, 241.

⁴³ Once Henry VII’s right to rule was established, he showed no hesitation in using the fiction of the three estates when it suited his convenience: for example, the renewal in 1499 of the Treaty of Étapes of 1492 in which the king’s letters patent to Louis XII of France state that “ratification . . . has been made by the three estates of the realm of England of the said peace.” In fact, though, the signatories were simply thirty-five members of the king’s whole or great council, parliament not then being in session. *Letters and Papers Illustrative of the Reigns of Richard III and Henry VII*, ed. James Gairdner (London, 1863), 2: 338.

archbishop at his coronation speak of his election by the three estates of the realm as the revisers of Richard III's coronation order had proposed. But to prevent any ambiguities about the validity of his right to rule, "the communities [*communitates*] of the realm of England presented to the king in parliament" a bill in English. It consisted of a single, long, but very straightforward, sentence:

To the pleasure of Almighty God, the wealth, prosperity, and surety of this realm of England, to the singular comfort of all the king's subjects of the same, and in avoiding of all ambiguities and questions, be it ordained, established, and enacted, by authority of this present parliament, that the inheritance of the crowns of the realms of England and of France, with all the preeminence and dignity royal to the same pertaining, . . . be, rest, remain, and abide in the most royal person of our now sovereign Lord King Harry the VIIth, and in the heirs of his body lawfully comen, perpetually with the grace of God so to endure, and in none other.

This bill was read, heard, and understood "by the assent of the lords spiritual and temporal, being in the said parliament, at the request of the said community, and by authority of the same parliament." Henry VII's answer, in French, was that the king "wills it in all points."⁴⁴ Thus did Henry VII seal the bond between the kingdom and the crown: in political theory through the three estates of the realm, but in political practice through parliament. This contract, based solely on an act of parliament, and not on any appeal to the estates meeting "out of" parliament, gave to the king and his lawful heirs the right to rule. Here, surely, was a true and proper parliamentary title for the first of the Tudor dynasty. Its wiser members, despite their intimacy with the Deity, were not to forget that theirs was a dynasty made-in-parliament and that they needed that high court's collaboration to rule with success.

By 1485, then, this series of five depositions had contributed greatly to Englishmen's understanding of the authorities by which they were ruled. Under normal circumstances their governance was primarily regal, but in the process of removing unsatisfactory kings, men of politics and law gradually set limits beyond which a reigning monarch could not safely go. Otherwise, he risked losing the support of those men whose approbation, as expressed through their aid, counsel, and consent, he needed to rule. These limits took shape during the course of the first three depositions—in 1327, 1399, and 1461—at each of which the right to depose the king was predicated on definitions of the Bad King (one who had violated England's law, custom, and morality) and/or the Useless King (the incompetent executive and inept politician who had not mastered the art of handling men). And even though Edward V's principal defect in 1483, his non-age, did not fit this emerging pattern, two years later Henry VII demonstrated that these definitions had won a permanent place in public law. For the first Tudor king was not content to leave the judgment to God alone, as rendered at Bosworth, but was

⁴⁴ *RP*, 6: 270; cf. *Statutes of the Realm*, 2: 499; S. B. Chrimes, *Henry VII* (London and Berkeley, 1972), 60, n. 1, states that Richard III's coronation oath was "presumably the same as that taken by Henry VII."

quick to bring a bill of attainder proclaiming “the unnatural, mischievous and great perjuries, treasons, homicides and murders, in shedding of infants’ blood, with many other wrongs, odious offenses and abominations against God and man, and in especial our said sovereign lord, committed and done by Richard, late duke of Gloucester, calling and naming himself by usurpation King Richard the Third.”⁴⁵ So began the legend of bunch-backed Richard the monster, but there was in the beginning of this myth also a summing up: one in which, through five depositions, bad history had made good law.

Furthermore, these depositions led Englishmen better to understand the circumstances under which a king could lose his right to rule. They served also to underscore a gradual, though nonetheless remarkable, change in men’s thinking about the authority needed to sanction a new regime. At all five depositions the new king and his advisers made every effort to justify his title and his right to rule. Many of their claims were traditional—the standard appeal to a true inheritance from a once-reigning sovereign; the equally universal sanction derived from God’s approval as indicated by military victory (at Towton and Bosworth) and by His sacraments of anointing and crowning. Conquest and force, though often the practical reality among these descendants of William the Bastard, were scarcely mentioned in their apologies. In politics, might may have made right, but in law, few men were eager to assert that brute strength provided their ultimate justification.

Instead, men of politics and law, and the new kings they supported, came increasingly to make their final appeal to the authority of the kingdom. In 1327 this authority appeared only in references to the *vox populi* and “the whole community of the kingdom.” By 1399 it had grown to the point where “the estates and the people,” who constituted the kingdom and implicitly possessed its authority, could authorize the deposition of one king and the election or choice of his successor. By 1460 the legal fiction of the estates of the realm, now three in number, had begun to fuse, albeit tentatively and under pressure of extreme circumstances, with parliament and its institutional authority. This process continued and took near-final form in 1483–84 when Richard III discovered that his own election by the three estates, to have both practical and constitutional validity, could not take place *out of* parliament, only *in* it. This development showed that, despite the continuing need for royal writs and regal ceremonial, parliament’s authority now derived from that of the kingdom; and hence that in politics, if not in law, that body had become transcendent. Thus by 1485 five depositions and Henry VII’s Act of Succession had somewhat dimmed those “sparkles of the Divinity” with which King James I attempted to adorn his imperial person before parliament in 1605.⁴⁶ In their stead, many an Englishman saw the glittering spangles of nationality that had begun to embellish the king’s high court of parliament, now also the kingdom’s representative.

⁴⁵ *RP*, 6: 276.

⁴⁶ *The Political Works of James I*, ed. Charles H. McIlwain (Cambridge, Mass., 1918), 281.

Art and Politics in Cold War America

JANE DE HART MATHEWS

PUBLIC PATRONAGE OF THE ARTS, instituted during the depression as a New Deal measure to provide work for the unemployed, was revived in the Cold War years as part of America's struggle against communism. From this fusion of art and politics issued a form of "free world" advertisement—the cultural exchange program. Ironically, however, the program that was intended to demonstrate that the United States was not a nation of materialists ran afoul of people who were themselves devoted opponents of Communist aggression, but who believed that this effort, in the visual arts especially, contained disturbing evidence of domestic subversion. Recognizing the power of art as a medium of influence and mode of communication, ardent anti-Communists in Congress, notably Michigan Congressman George Dondero, supported by local counterparts, inveighed against works reflecting social commentary that they deemed subversive, whether in an overseas embassy or a local post office, with a zeal scarcely matched since the thirties. Lambasting individual artists who allegedly had Communist associations, they demanded that works by such artists, no matter how innocuous the subject matter, be banned from publicly supported arts institutions and most especially from federally sponsored cultural exchanges. Finally, they attacked modern art itself as an instrument of Communist subversion in terms that blended fact with fantasy.

These three forms of expressing political discontent by focusing on art were really three stages of one process. The initial stage, opposition to social commentary in predominantly representational art, involved at first little more than an objection to the particular message conveyed by artists who used their work to communicate what they perceived as social injustice. The second stage, objection to the political affiliations of the artists, involved a more complicated response because it attempted to link ideology and art in the person of the artist irrespective of the content of specific works. The objection to modern art as Communist conspiracy, the final stage, involved a yet more "sophisticated" thought process, for the assumption was that rejection of traditional ways of seeing form and space inherent in vanguard style of painting implied rejection of traditional world views. Iconoclasm in art, in

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other words, was assumed to extend to the broader realms of the cultural and social order, threatening ultimately all established norms and values.

Most Americans, to be sure, probably did not like or understand modern or abstract art; but most of them, too, could simply “take it or leave it.” And certainly most informed opponents of the form would never identify it as part of a Communist conspiracy. Indeed, most Americans would not confuse their esthetic judgment or preferences with political commitments; but some people would do so in a public and dramatic way. And they would demand that others follow their lead in making deviation from their own esthetic canon, not a matter of taste, but of deviation from their standard of “Americanism.”

Although such a peculiar conflation of politics and esthetics was commonplace in the United States during the late forties and fifties, it would be a mistake to view the assault on modern art solely in political terms, for the inability to tolerate new esthetic values and modes of perception reveals much about the psychological sources of antiradicalism in this country as well as the symbolic significance of public patronage. Since congressional critics were aided by certain representational artists who feared the growing ascendancy of abstract expressionism, their attacks also illuminate the dynamics of the modernist-traditionalist controversy that flared up again in the fifties: politics not only became esthetics but esthetics became politics. To focus on those anti-Communists who made internal subversion a political issue and their alliance with disgruntled traditionalist painters and sculptors, however, is not to suggest a simplistic or invariable correlation between political and esthetic traditionalism—or conversely, political and esthetic radicalism. Rather it affords a unique opportunity to probe some of the pressure behind the impulse to prevent certain modes of art from being styled as “American”—indeed, to label them as “un-American.” More specifically, by focusing on this unique esthetic-political continuum, it is possible to show why modern art, which Communists themselves hated and feared no less than their American enemies, should have become emblematic of “un-Americanism”—in short, cultural heresy.

While opposition to works of art never assumed the proportions in this nation that it did in totalitarian countries, where offending artists often suffered imprisonment as well as loss of official patronage, the impulse to censor was undeniably present in the United States in the Cold War years. The simplest and most direct expression of that impulse involved opposition on political rather than esthetic grounds. Critics objected to the message communicated through immediately perceptible images conveying the artist's convictions about past injustices and present wrongs. Predictably, therefore, criticism developed about the social commentary inherent in such works as Ben Shahn's *Hunger* and Robert Gwathmey's *Work Song* when, in 1946, the State Department purchased its first collection of art for exhibit abroad. But the paintings that drew sharpest attack were not part of that ill-fated collection; rather, they were a part of San Francisco's Rincon Post Office Annex murals.

The controversy, like the murals themselves, was reminiscent of the thirties. One of the last and most expensive (\$26,000) of the mural projects initiated by the Treasury Department's Section of Fine Arts, this 240-foot painting depicting the history of San Francisco was the work of Anton Refregier. An established muralist, whose sketches had been chosen from among those submitted by eighty-one competing artists, Refregier, like most socially conscious artists in that depression decade, believed that art must address itself to contemporary issues and that a mural painting in particular must not be "banal, decorative embellishment," but a "meaningful, significant, . . . powerful plastic statement based on the history and lives of the people."¹ His preliminary designs reflected that conviction. Although the jury approving the sketches praised the coherence of the historical element, federal officials were ever mindful that these were public buildings being decorated with public funds.² In such circumstances, the mood was one of caution: artistic freedom had to yield to public accountability. No artist, however distinguished, escaped the "heavy, if well meaning, hand" of federal supervision.³

Refregier was no exception. There was the usual haggling with officials of the Public Buildings Administration about minor details in the murals as work was resumed after the war. A VFW cap had to be painted out since veterans' groups had protested the presence of a man with such a cap in a strike scene—newspaper pictures to the contrary notwithstanding. By the same token alteration had to be made in a scene that depicted a parade celebrating the winning of the eight-hour day. Officials also objected to the inclusion of a huge head of FDR in a panel on the Four Freedoms, despite the artist's protest that a portrait of the late president was not only historically appropriate but a necessary focal point.⁴ But such changes, while not uncommon on mural projects of the thirties, were insufficient to stifle criticism from civic groups, veterans' organizations, and patriotic and fraternal societies in a nation where concern about Communist subversion had been intensified by the Hiss trial and the "fall" of China.⁵

When the completed project was finally unveiled in 1949, the L-shaped

¹ Anton Refregier, *Government Sponsorship of the Arts* (New York, 1961), 7. Quoted in Gladys M. Kunkel, "The Mural Painting of Anton Refregier in the Rincon Annex of the San Francisco Post Office, San Francisco, California" (master's thesis, Arizona State University, 1969), 27.

² Kunkel, "Rincon Annex Murals," 33. Under terms of his contract, Refregier was to revise his preliminary designs until they met the requirements of the Commissioner of Public Buildings, and he was not to proceed with the execution of the murals until the commissioner or his duly authorized representative had given approval.

³ Richard D. McKinzie, *The New Deal for Artists* (Princeton, 1973), 60-61.

⁴ For details of the VFW controversy, see the *San Francisco Chronicle*, May 17, 1948, and the *San Francisco Examiner*, Oct. 27, 1948. The dispute over the eight-hour day celebration is discussed in William Hauptman, "The Suppression of Art in the McCarthy Decade," *Artforum*, 11 (1973): 52. For the Roosevelt portrait controversy, see the *Examiner*, Nov. 14, 1947, and also Refregier's letter to Gladys M. Kunkel quoted in her "Rincon Annex Murals," 166. For additional details see Matthew Josephson, "The Vandals are Here: Art is Not for Burning," *Nation*, 177 (1953): 244-48; and Amy Robertson, "Refregier Paints a Mural," *Art News*, 48 (1949): 32-34.

⁵ Opponents included the California Council of Republican Women, the DAR, the Native Sons of the Golden West, the American Legion, the VFW, the Sailors' Union, and the Society of Western Artists, a traditionalist group made up largely of commercial illustrators.

lobby of the Rincon Annex was panelled with a series of vibrantly colored, generally well-designed and executed paintings that carried San Francisco history from exploration and conquest, past the gold rush, the building of the Union Pacific, the disastrous earthquake and fire, and into the twentieth century and a second World War, culminating in the signing of the UN charter.⁶ Delighted with their artistic quality, the San Francisco Art Association called Refregier's work "among the most distinguished mural paintings executed in this country in the past generation."⁷ But other critics, perhaps accustomed to the noncontroversial character of most of California's New Deal art, found little to admire about the muralist or his murals.⁸ Refregier, a United States citizen since 1930, was naturally suspect because of his Russian background and myriad leftist activities.⁹ As for the murals, all twenty-nine panels were done in a kind of stylized realism that resulted in simple, elongated angular figures disturbing to many viewers—"Frankenstein monsters" was the appellation of a disappointed legionnaire who lamented that such art could never be "pleasing and uplifting."¹⁰

If style was offensive, content was more so. Denouncing the paintings as "artistically offensive," "historically inaccurate," and "subversive," opponents charged that they cast a "derogatory and improper reflection" on the character of the great state of California.¹¹ Specifically critics objected to scenes dealing with the California fire and earthquake, vigilante activity, Chinese coolie labor, the Mooney case, and labor difficulties on the waterfront. Such subjects, they complained, placed disproportionate emphasis on violence, racial hatred, and class struggle. Detail was as subversive as the general theme. Making monks "cadaverous" or "potbellied" was anticlerical; putting the British flag above the American flag in a panel on World War II was unpatriotic; painting a hammer and sickle on the Soviet flag in the scene depicting the signing of the UN charter was subversive, and portraying the U.S. representative with "mule's ears" was worse still. So the list went. With even San Francisco's Young Democrats insisting that the presence of such murals in a public building was "little short of treason," public officials responded accordingly.¹² In a letter to a California veteran, Representative Richard M. Nixon had previously stated that, while he realized that "some very objectionable art of a subversive nature" had been allowed to go into federal buildings in many parts of the country, he believed that there was no remedy under the present administration. As [*sic*] such time as we may have a

⁶ For a fuller description see Kunkel, "Rincon Annex Murals."

⁷ *San Francisco Chronicle*, Mar. 10, 1953.

⁸ With the exception of the Coit Tower murals that included certain radical symbols, most federally subsidized art in California during the thirties was "overwhelmingly bland and noncontroversial," according to Steven Gelber. See his "New Deal Art in California" (paper presented at the Apr. 1976 meeting of the Organization of American Historians at St. Louis).

⁹ *Congressional Record*, 82nd Cong., 2nd Sess. (1952), A3765; *San Francisco Chronicle*, May 21, 1953.

¹⁰ Charles E. Plant to W. E. Reynolds, Aug. 28, 1950, George Dondero Papers, Archives of American Art (Washington, D. C.).

¹¹ *San Francisco Chronicle*, May 2, 1953; *San Francisco Examiner*, May 23, 1953.

¹² Accusations are detailed in Aline B. Louchheim's articles in the *New York Times*, May 2, 10, 21, 1953, and Alfred Frankenstein's in the *San Francisco Chronicle*, Mar. 10-13, Apr. 3, May 2, 1953.

change in the administration and in the majority in Congress,” he wrote, “I believe a Committee of Congress should make a thorough investigation of this type of art in Government buildings with a view to obtaining removal of all that is found to be inconsistent with American ideals and principles.”¹³ Nixon’s fellow California representative, Hubert B. Scudder, agreed.

By 1953, the conditions that Nixon believed necessary obtained: elections the previous fall had swept into office a Republican president and Congress. More important, the Chairman of the House Committee on Public Works was George Dondero. A Michigan Republican who frequently denounced Communism in the arts, Dondero promptly appointed a subcommittee of the Committee on Public Works to consider removal of murals that he considered “an insult to every loyal American.”¹⁴ Accordingly, Scudder introduced H.J. Resolution 211, which directed the Administrator of General Services to remove the twenty-nine murals on the grounds that: 1. they were derogatory to and an improper reflection on the character of California’s pioneers; and 2. they contained Communist propaganda. In hearings before the House subcommittee, Scudder was supported in his effort by a colleague from the House Un-American Activities Committee who read at length from committee files of the many instances when Refregier had lent his name to publications, resolutions, congresses, and organizations listed by the Attorney General as subversive. The Commissioner of Public Buildings, who was responsible for awarding the contract to the Russian-born artist, explained that he personally had never liked the sketches. Most damning, however, were Scudder’s own remarks. Charging that the murals were not only historically inaccurate, but “slandrous” and “subversive,” he inserted into the record supporting statements from critics charging that the murals failed to depict “the romance and glory of the West” and tended instead to promote “racial hatred and class warfare.” Other witnesses seconded his objections, agreeing that the murals did indeed project Communist propaganda.¹⁵

Opponents of removal had also come prepared. California Congressmen John Shelly, a liberal Democrat, and William Maillard, a moderate Republican, were both outspoken in opposing the resolution of their conservative colleague.¹⁶ Attesting to the accuracy of the murals, Shelly readily admitted that a great deal of California’s early history was not to his liking. The Chinese labor riots and the Mooney bombing were historical facts, he said. Moreover, this was not the USSR, where artists were judged on their political views. Representatives of the Bay Citizen’s Committee to Protect the Rincon Annex Murals and various allied groups such as the San Francisco Museum

¹³ Richard M. Nixon to Charles E. Plant, July 18, 1949, Hudson Walker Papers, Archives of American Art. Nixon appears to have reversed himself somewhat in 1955. When a lithograph called “Dick McSmear” was removed from the San Francisco Art Festival, Nixon wired that city’s Art Commission on behalf of the artist’s freedom of expression. See *Art News*, 54 (1955):7.

¹⁴ George Dondero to the Michigan American Legion, Dec. 1, 1956, Dondero Papers.

¹⁵ Hearings before the House Subcommittee on Public Buildings and Grounds of the Committee on Public Works on H. J. Res. 211, *Rincon Annex Murals*, San Francisco, 83rd Cong., 1st Sess. (Washington, 1953), 1-42.

¹⁶ See Congressional ratings, published in the *ADA World*, 1949-53.

and the San Francisco Art Association, were also on hand to rebut Scudder's charges. The Committee, consisting of what the San Francisco *Chronicle* characterized as an "imposing array" of leaders in business, finance, the arts, and society, had been organized in part through the efforts of Grace McCann Morley, Director of the San Francisco Museum of Art. She was convinced of the flimsy basis of the arguments for removal and the alarming precedent that it would afford.¹⁷ Speaking first for the Committee, Attorney Chauncey McKeever submitted a nine-page statement condemning the resolution. Citing testimony of authorities in the California Historical Society, he argued that the murals were historically accurate as well as esthetically sound and politically safe. Though certain minor changes might be desirable in a few panels, such as the UN scene, to pass the resolution, he insisted, would be to yield to the prejudice of uninformed pressure groups. Thomas Carr Howe, Jr., Director of the California Palace of the Legion of Honor, agreed, as did representatives of the American Federation of Art and Artists Equity. From these and other nationally based artists' groups the message was the same: regardless of style, subject matter, or the political beliefs of the artist, to destroy a work of art was to destroy freedom of expression.¹⁸

In the days that followed, supporters of the murals continued to plead their case. In San Francisco, the *Chronicle* not only covered the controversy fully, devoting substantial space to a detailed statement attesting to the historical veracity of the episodes Refregier had chosen to portray, but also editorialized on behalf of preservation, suggesting that the Scudder resolution was reminiscent of events in totalitarian societies.¹⁹ And in Washington, the House subcommittee was inundated with mail—some of it advocating the destruction of murals that functioned as "an instrument of Soviet psychological warfare," but much of it arguing for preservation of important works of art. Written not only by ordinary San Franciscans, but also by museum directors and art experts across the country, the letters attacked removal as "official cultural vandalism." Arguing on behalf of artistic freedom, Lloyd Goodrich of the Whitney pointed out that opposition to the murals had come from only "a few reactionary elements" in California politics and art. Julian Huxley, former Director-General of UNESCO, deplored such "witch-hunting" in the United States. The murals' removal, he said, was an "example of the growing tendency" in this country "to exert political control over freedom of thought and expression." And from Artists Equity came the reminder that passage of

¹⁷ San Francisco *Chronicle*, May 1, 1953; Grace Morley to Jane De Hart Mathews, Aug. 19, 1975. Indicative of the insubstantial character of the charges against the murals was the assertion that the artist had painted mule's ears emanating from the head of the U.S. representative in the UN panel. But when the painting was examined *in situ*, the forms in blue behind the figure of the American representative which might conceivably be construed as mule's ears in a photograph of the panel were quickly recognized as swags of the blue drapery that decorated the stage of the San Francisco Opera House, where the Plenary Sessions of the UN were held. Morley's efforts on behalf of the murals are documented in letters such as that of Morley to J.D. Zellerbach, Apr. 10, 1952, Hudson Walker Papers. Zellerbach, who was president of the Crown-Zellerbach Corporation, was only one of the many corporate leaders whose support she sought. For a complete listing of the supporters of the Citizen's Committee, see the *Chronicle's* May 1 article.

¹⁸ *Rincon Annex Murals*, 43ff.

¹⁹ San Francisco *Chronicle*, May 1, 18, 1953.

the Scudder resolution might deal an irreparable blow to America's cultural standing abroad at precisely the time federal officials were trying to improve that standing through cultural exchanges.²⁰

The issue was clearly complicated, because Refregier, ever willing to use his art as political commentary, had deliberately chosen to depict controversial issues such as the Mooney case and the anti-Chinese riots in the conviction that the public must be reminded of the "unfortunate aspects" of its history if such events were not to be repeated.²¹ In the end, however, the subcommittee ruled that removal was an administrative matter, and since the Commissioner of Public Buildings insisted that the murals could not be taken down without explicit authorization from Congress, they remained intact. Still, efforts at censorship persisted, and as late as 1956 California veterans continued to explore strategies for the removal of the murals.²² That they persisted was a measure of the intensity of feelings aroused when art was used by social realists to express the artist's hatred of injustice. That they ultimately failed was a result, at least in part, of the effectiveness with which prominent San Franciscans mobilized themselves and their allies on behalf of artistic freedom, for without such action, anti-Communist crusades leading to patriotic excesses could fast gain momentum in the deteriorating political climate of the early fifties.

Fed by frustrations with a U.S. foreign policy that failed to "win" diplomatic victories against the Soviet Union, these crusades became increasingly concerned with internal subversion. Predictably, this effort to ferret out the "enemy within" led to persons who had participated in groups, activities, or causes sponsored by "Communist-front" organizations during the thirties. Artists were naturally among these suspected subversives, not only because of the content of specific works, but also because of past personal or professional associations. Thus self-styled anti-Communists, operating at this second level of sophistication, attempted in the course of such attacks to unite ideology and art through the person of the artist. Events in Dallas, Texas provided a case in point. That this burgeoning center of new wealth should have become the scene of controversy is hardly surprising. The headquarters of millionaire H. L. Hunt, the city had been exposed to his national radio and television broadcasts as well as numerous publications such as the *American National*

²⁰ I am especially indebted to the Chairman of the House Committee on Public Works for special arrangements to read at the Committee office all correspondence relating to the Scudder resolution. The material, "Considerations of H. J. Res. 211, 83rd Cong., 1st Sess. (1953): 'To direct the Administration of General Services to remove the mural paintings from the lobby of the Rincon Annex Post Office Building, San Francisco, California,'" although a part of Record Group 121 at the National Archives, is not otherwise available. "The Rincon Annex Murals: A Psychological Warfare Criticism," a ten-page typed manuscript, is contained in the files, as are the other arguments referred to above in letters from: Alfred H. Barr, Jr., Apr. 21, 1953; Lloyd Goodrich, Apr. 29, 1953; Julian Huxley, Apr. 9, 1953; Lincoln Rothschild, Apr. 22, 1953.

²¹ Interview with Anton Refregier, Jan. 2, 1975. I am grateful to Helen A. Harrison who conducted the interview for providing me with a transcript.

²² George Dondero to Charles E. Plant, June 29 and Nov. 19, 1956; Dondero to Arthur E. Summerfield, Nov. 19, 1956; Dondero to the Michigan American Legion, Dec. 1, 1956, Dondero Papers.

Research Report, which “named” Communists and “Communist-front” organizations, describing their activities in detail.²³ Another Dallasite, Colonel Alvin Owsley, had begun compiling his own directory in 1921, which, with names from the annual reports of the House Un-American Activities Committee and congressional speeches, constituted a formidable list of “subversives” against which local citizens could check the names of writers, composers, actors, and artists that appeared in local newspapers and museum catalogs.²⁴ Although certain names had involved the Dallas Museum in a variety of conflicts, the incident that brought the loudest reverberations was the showing of “Sport in Art.”²⁵

Assembled by the American Federation of Art for *Sports Illustrated*, the exhibit was to tour the United States during 1956 before going to Australia under USIA auspices for the Olympics. Two months before its arrival in Dallas, however, local anti-Communists demanded the exclusion of four paintings because of the alleged subversive associations of the artists involved. The four included Ben Shahn’s drawing of a baseball game entitled “National Pastime,” a skating scene by Yasuo Kuniyoshi, another winter scene with ice skaters by Leon Kroll, and a painting of an elderly fisherman by William Zorach.²⁶ Although Shahn’s work, like that of other Social Realists, often served as a means of communicating the artist’s political and social values, these particular paintings bore no “message.” But however innocuous the works themselves, all four artists had been linked to “front” organizations in the thirties. According to the House Un-American Activities Committee, Shahn, for example, had associated with numerous radical groups, publications, and causes, among them the John Reed Club, the American Artists Congress, the Spanish Refugee Relief Committee, *Masses and Mainstream*, and the National Council of American-Soviet Friendship.²⁷ Museum trustees nevertheless issued a statement saying that the museum acquired and exhibited works solely on the basis of merit.²⁸ Supporters also pointed out that the exhibit, which would be cosponsored by Neiman-Marcus, cost local taxpayers

²³ Charlotte Devree, “The U.S. Government Vetoes Living Art,” *Art News*, 55 (1956), 35.

²⁴ *Ibid.*

²⁵ *Ibid.* According to Devree, the Public Affairs Luncheon Club, a women’s group of some 400 members, had previously taken the Dallas Museum to task for its modernist tendencies and for its exhibitions of work by artists with “known” Communist affiliations. Under the leadership of Stanley Marcus, owner of Neiman-Marcus, the museum’s trustees initially resisted the demands of these critics, only to capitulate when Marcus’ successor was in charge. The latter called on Congressman Dondero for advice, circulated lists of “subversive” artists among museum trustees who, in turn, issued a statement that it was not the museum’s policy knowingly to acquire or exhibit works by persons known to be Communist or to have “front” affiliations. Not surprisingly, two subsequent exhibitions came under attack: “In Memoriam” because twelve of the artists, all recently deceased, had “leftist associations”; and “Sculpture in Silver” because of William Zorach’s participation. Neither exhibit was federally funded.

²⁶ *Ibid.* The controversy was followed closely by the *Dallas Morning News*, Feb. 1, 3–5, 7, 23, 29, 1956, and the *New York Times*, Feb. 12 and Mar. 4, 1956, sec. 2.

²⁷ Attacks on Shahn’s leftist associations, which may be found in Dondero’s early speeches, continued through the “Sport in Art” controversy and culminated in an investigation by the House Un-American Activities Committee. See *Congressional Record*, 81st Cong., 1st Sess. (1949), 11586, and Hearings Before the House Un-American Activities Committee. *The National Exhibition, Moscow*, 86th Cong., 1st Sess. (Washington, 1959), 941–50. Cited hereafter as HUAC Hearings.

²⁸ *Dallas Morning News*, Feb. 1, 1956.

nothing.²⁹ But the museum's critics were in no way mollified. Convinced that the trustees were little better than Communist "dupes," and knowing that public funds were funneled through the Park Board to the museum for maintenance, they appealed to the Park Board to ban the exhibit.³⁰ In subsequent hearings both the identity of these self-styled patriots and the nature of their argument became apparent.

Opponents of the museum's policy were led by the Communism in Art Committee of the Dallas Patriotic Society. The society itself consisted of a federation of some sixteen clubs, principally veterans' groups, patriotic associations, and an assortment of local art clubs.³¹ Their presence in the Patriotic Society owed much to the energetic efforts of Reveau Basset, a local resident and traditionalist artist, who frequently spoke to groups of women amateur painters about the dangerous modernist tendencies of the Dallas Museum.³² By failing to remove the works of Communist artists, they charged, the Dallas Museum was aiding communism, inasmuch as exhibitions in a leading museum gave the artist standing that could be translated into monetary terms. And money, of course, could be contributed to Communist causes that in turn would harm America. There was, spokesmen for the Patriotic Council concluded, a "well-organized apparatus operating to exhibit the works of Communists, however inferior, in preference to the works of others, however superior."³³ Unconvinced, the Park Board rejected the request to withdraw "Sport in Art" amidst mutterings about its members' having gone soft on communism.³⁴ The exhibit went on as scheduled, though critics stationed themselves beside the pictures in question to warn the public that these were the works of "Reds."³⁵ But any notion that artistic freedom had triumphed was short-lived.

The clamor of the Patriotic Council in Dallas had repercussions in Washington. Fearing congressional criticism, USIA Director Theodore Streibert reversed the decision to send "Sport in Art" to the Olympics. Moreover, he promptly cancelled an important overseas exhibit of twentieth-century American painting after the American Federation of Art (AFA), which had assembled the exhibit, refused to remove the works of ten artists whom the Agency deemed "social hazards" by virtue of their "front" associations.³⁶ This cancellation, however, was simply the latest in a long line of aborted exhibitions. The State Department had called back an exhibition of American painting as

²⁹ *Ibid.*, Feb. 4, 23, 1956.

³⁰ *Ibid.*, Feb. 7, 1956.

³¹ Included were the DAR, Daughters of 1812, American Legion, VFW, Public Affairs Luncheon Club, Pro-America, 1950 Study Club, Southern Memorial Association, Inwood Lions Club, Matheon Club, Reveau Basset Art Club and Reaugh Art Club.

³² Devree, "The U.S. Government Vetoes Living Art," 35.

³³ *Dallas Morning News*, Feb. 3, 7, 23, 1956. The leading speech at the Park Board hearing was made by Colonel Alvin Owsley. Other speakers included Colonel John Mayo and B. I. F. McCain, also a Legionnaire and a past president of the Dallas Chamber of Commerce. There is evidence to suggest that Owsley's speech may have been written by a Dallas artist.

³⁴ *Dallas Morning News*, Feb. 29, 1956.

³⁵ Devree, "The U.S. Government Vetoes Living Art," 56.

³⁶ *Washington Post and Times Herald*, June 22, 1956.

early as 1947, fired the young man who had put it together, and sold off the paintings at auction amidst charges from Congress and the press that twenty of the forty-five artists represented “various shades of Communism.”³⁷ Mindful of this history, the AFA had hoped to avert this latest cancellation by persuading the USIA director to refer the matter to the White House. Eisenhower had issued a declaration on artistic freedom two years earlier as a result of the efforts of Alfred Barr and trustees of the Museum of Modern Art, who had used the Museum’s twenty-fifth birthday celebration to elicit a presidential statement.³⁸ The hope was that it might now become more than a paper proclamation. But the AFA was quickly disappointed. Sherman Adams, the president’s chief assistant, supported Streibert’s decision.³⁹ In discussing his action, the USIA director explained that if he regarded the artists’ politics as irrelevant he would surely face congressional criticism and perhaps jeopardize appropriations for the entire agency.⁴⁰ Accordingly, he also cancelled an exhibit put together by the College Art Association because of the inclusion of a Picasso. And in a similar display of caution, the State Department revoked plans for an overseas tour of Toscanini’s former NBC Symphony of the Air because 4 of the 101 members of the orchestra were allegedly pro-Communist. Administration “cowardice” was attacked on the Senate floor by Hubert H. Humphrey and on the editorial pages of the *New York Times* and the *Washington Post*.⁴¹ In the context of the political climate of the fifties, however, such caution was perhaps understandable.

Though by 1954 Joseph McCarthy’s fellow Senators had voted to censure their colleague, the crusade that bore his name had not yet abated. Fears over national security and internal subversion as well as frustrations over the cost and complexity of the Cold War persisted during the Eisenhower years, prompting the administration to extend the loyalty/security programs instituted in the Truman era. Thus preoccupied with the paraphernalia of those programs—the Attorney General’s list, the loyalty oaths—a bureaucratic organization such as the USIA was not prepared to invite the suspicions of those congressmen who were ever ready to use appropriations as a coercive tool with which to enforce their own definition of loyalty.

Many ardent anti-Communists were bothered, however, by more than “Marxist” social commentary in art and “subversive” political associations of

³⁷ See below, footnotes 54 through 57.

³⁸ Lloyd Goodrich interview, Archives of American Art. The American Federation of Arts, which assembled many of the exhibits circulated abroad by the federal government, had issued its statement that same month. See footnote 17.

³⁹ Goodrich interview.

⁴⁰ In one of his many speeches attacking government involvement with “subversive” artists, Dondero praised Dallas citizens for their efforts to prevent the showing of “Sport in Art.” He would certainly have taken to task the USIA had the exhibit been sent abroad as planned. See the *Cong. Rec.*, 84th Cong. 2nd Sess. (1956), 10419-25, 13774-849.

⁴¹ Humphrey was one of the first—and few—Senators to speak out against cancellations. See the *Cong. Rec.*, 84th Cong., 2nd Sess. (1956), 10918-20, 12305-6. Senator William Fulbright also inserted into the *Record* editorials criticizing the USIA from the *Times* and the *Post*. (See A5602-3.) With the exception of Jacob Javits, most congressmen were silent throughout the decade, taking what Humphrey called in reference to his Senate colleagues a stand “a little to the left of Grant or McKinley.”

artists. Their *bête noire* was modernism, for the rejection of traditional forms and the commitment to abstraction that characterized vanguard art seemed to impart to their highly structured world the quality of chaos and the demonic that they so easily identified with communism. Thus the charges that the promotion of modern art was part of the Communist conspiracy represented the third and most “sophisticated” stage of the argument of those who labeled any deviation from their own esthetic canon, not a matter of taste, but a deviation from their standard of “Americanism.”

A number of congressmen were hostile to modern art, but the representative who most consistently and thoroughly emphasized its intrinsic un-American character was Michigan’s George Dondero. A slender, erect man with regular features and thinning gray hair, Dondero inveighed for the better part of a decade against everything from State Department exhibits to artists’ unions. The real enemy, however, was modern art and those “misguided disciples who bore from within to destroy the high standards and priceless tradition of academic art.” With the closely reasoned rhetoric so characteristic of conspiratorial thinking, Dondero argued that modernism had been used against the Czarist government when Trotsky’s friend, Wassily Kandinsky, had released on Russians “the black knights of the isms”: cubism, futurism, dadaism, expressionism, constructionism, surrealism, and abstractionism. Each was deadly. Cubism, according to Dondero, aimed to destroy “by designed disorder”; futurism, “by the machine myth”; dadaism, “by ridicule”; expressionism, “by aping the criminal and insane”; abstractionism, “by the creation of brainstorm”; surrealism, “by the denial of reason.” To be sure, socialist realism ultimately prevailed in Russia itself. But the art of the Revolution had been cleverly retained for subversion abroad. Thus from the “pen-and-brush phalanx of the Communist conspiracy” had come the “front” organizations of the thirties: the John Reed Clubs, the League of American Writers, the American Artists Congress, and their successors, such as Artists Equity.⁴²

Current targets, according to Dondero, were America’s cultural centers, which were being infiltrated by modernism with all its “depravity, decadence, and destruction.” The Museum of Modern Art was a prime example since its origins went back to the Société Anonyme of which Kandinsky himself had been an officer. Nor had Kandinsky been the only revolutionary apostle of modernism invading America to corrupt its art. A “horde of foreign art manglers” had descended upon this country just before World War II, spreading their pernicious doctrines. Followers of these “international art thugs” now included Americans such as Robert Motherwell, William Bazotes, and Jackson Pollock. Thus as followers increased and as American universities turned out museum directors sympathetic to modernism, the danger from foreign “isms” increased proportionately. Harvard’s Fogg Museum, Dondero charged, was one of the worst offenders. The “effeminate

⁴² *Cong. Rec.* 81st Cong., 1st Sess. (1949), 11584.

elect" it sent out to run the nation's museums not only discriminated against traditionalists in juried shows, but also paid inflated prices for modern art that they jammed down the throat of an unwilling public. In sum, a "sinister conspiracy conceived in the black heart of Russia" had become a threat to America's cultural institutions and to those loyal American artists who sought to protect their cultural heritage from the new forms that were the symbols of a foreign ideology. Only if the hard-working, right-thinking patriotic proponents of academic art followed the example of labor unions and ejected Communists, the Michigan Congressman argued, could traditional art be preserved and cultural institutions cleansed.⁴³

Simplistic and uninformed, such an attack can be temptingly dismissed as yet another example of what Richard Hofstadter has called the "paranoid style." However labeled, Dondero's protest and, more important, the tensions and anxieties behind it cannot be discounted, for to do so would be to ignore the concerns and influence of a fearful and militant minority. For at least some Americans Dondero's was an appealing argument. As in most conspiracy theories, there were sufficient elements of truth to elicit belief from those people who seek to escape the complexity of causation by devising simplistic, face-saving formulas wherein fears can be assuaged and anger externalized. Such people may also have resonated to the anti-intellectual and frankly nativistic overtones of Dondero's address: the characterization of Harvard-produced museum directors as an "effeminate elect," the dubbing of this nation's newest emigrés as a "polyglot rabble." Certainly there was little question that Dondero's remarks struck a responsive note in people such as Wheeler Williams, the President of the National Sculpture Society and one of the "real" American artists whom Dondero called to take up arms against this "horde of foreign art manglers." Indeed, Williams publicly lamented the fact that the USIA would send abroad works by a "newly made American" such as Theodore Roszak, while ignoring such established sculptors as the late Daniel Chester French and Augustus Saint-Gaudens.⁴⁴

Undeniably the ethnic and social character of the American art community had changed radically in the early decades of the twentieth century, as first generation Americans, many of them immigrants, flowed into the New York art world. These were the "newly made" Americans—immigrant sons such as the social realist Jack Levine, who discovered art in a settlement house and fame on the WPA. And there were also the emigrés: the "notorious" French

⁴³ The traditionalist organizations that Dondero wanted to take action were the National Academy of Design, the American Artists Professional League, Allied Artists of America, the Illustrators' Society, the American Watercolor Society, and the National Sculpture Society. See *Cong. Rec.*, 81st Cong., 1st Sess. (1949), 6375. For a fuller exposition of his ideas on modern art, see *Cong. Rec.*, 81st Cong., 1st Sess. (1949), 11584-87; 84th Cong., 2nd Sess. (1956), 13774-79. See also his speech to the American Artists Professional League on March 30, 1957, "Communism and Art," Dondero Papers. According to American Federation of Arts representatives who spoke with Dondero, the Michigan congressman believed not only that museum juries were dominated by modernists, but that art journals and art critics of the metropolitan press were "undermining the standards of American culture." See Thomas Parker to [L.C.] Smith, memorandum, Hudson Walker Papers. In fairness, however, one must recognize the extent to which taste and esthetic criteria were and are affected by a few influential museum directors and critics.

⁴⁴ HUAC Hearings, 916.

surrealists, Tanguy, Masson, and Breton; the charismatic John Graham from Russia; Albers and Hofmann from Germany. On the eve of the Nazi invasion, others had followed, among them the great Dutch neoplastic painter and theoretician of abstract art, Piet Mondrian. Thus, by the mid-forties the emigré community virtually encompassed the leaders of every nonrealistic art movement of the twentieth century.⁴⁵ Modernist all, this “polyglot rabble” would help to change the nation’s artistic sensibilities far more radically than had a few *avant-garde* paintings in a New York armory some forty years before. But whether social realist or surrealist, such names and the art they represented were understandingly threatening to the eminently respectable academicians who dominated the National Sculpture Society, the American Artists Professional League, and the National Academy of Design. These groups had long enjoyed a monopoly on official art, and they did not choose to relinquish it. Indeed, the American Artists Professional League issued a “War Cry” against “decadent isms,” echoing Dondero in their plaint about the “sensationalists” who had “infiltrated *our* large exhibitions, *our* art societies, and *our* museums.”⁴⁶ Thus the final perhaps most important appeal was to traditionalists to reassert their hegemony in an art world that was once again caught up in the modernist-traditionalist controversy.

That controversy was, in fact, at the heart of this whole attack on communism in the arts. Raging intensely in the aftermath of the Armory show, then relatively quiescent in the thirties, it flared with renewed vigor against the backdrop of the McCarthy era. While congressional critics of modernism held forth in Washington, Pope Pius XII denounced surrealist and abstract art as “immoral” and “enslaving to the spiritual powers of the soul.” In Boston, the Institute for Modern Art changed its name to the less offensive Institute for Contemporary Art, defining neither term precisely, but in the process calling artists to come forward with a “strong, clear affirmation of humanity.” In New York, when the Metropolitan Museum included modernist work in a large retrospective of recent sculpture, conservative sculptors took the Museum to task for proving hospitable to a movement that “by destroying an ideal of beauty, endeavor, and discipline in artistic expression” threatened not only art but “the fundamental freedom of our American way of life.” At the White House, President Truman received assurances that hundreds of “stifled” realists could be rallied to paint patriotic pictures of America free from the distortion and disharmony inherent in the works of Communist-inspired modernists.

On the defensive once again, the modernists tried to counter the attack. At the New School for Social Research, where a curtain was subsequently placed over an Orozco mural entitled “Revolutionary Violence,” Stanley Hayter

⁴⁵ For a fuller assessment of the immigrants’ impact, see Jo Ann Lewis, “Immigrant artists: who they were and what they did,” *Smithsonian*, 7 (1976): 92–101. Visual evidence of their contribution is contained in the Hirshhorn’s exhibition, “The Golden Door: Artist-Immigrants of America, 1876–1976” (May 20–Oct. 20, 1976).

⁴⁶ See “War Cry,” a release of the American Artists Professional League. A copy is printed in HUAC Hearings, 906. Italics mine.

debated the whole modernist-traditionalist issue with fellow artist and social realist George Biddle. An advocate of socially conscious art and an ardent admirer of FDR, Biddle, like many traditionalists, was not conservative in either an esthetic or political sense. And he would never have equated modernism with communism. But he was adamant in his argument that nonobjective art would prove a short-lived fad. Its appeal, he insisted, was the product of "war neurosis, a dealer-rigged market, and snobbism." In the pages of the *New York Times* critics continued to explore the issue still further. Aline B. Louchheim had previously deplored the willingness of some traditionalists to make abstract moral judgments about work that was non-representational. Her colleague, Howard Devree, focused more precisely on charges of communism, pointing out that socialist realism, the objective style that dominated official art in the USSR, represented not modernism but its very antithesis. Moreover, he noted a dangerous parallel between the practices of totalitarian regimes under Hitler and Stalin and the current attacks on vanguard art as "degenerate" stuff deserving suppression and censorship. Alfred Barr, former director of the Museum of Modern Art, elaborated on this theme in a long article, "Is Modern Art Communist?" pointing out that, on the contrary, abstraction had been damned in Soviet Russia as decadent formalism since the early twenties. He might also have added that modernism had been subsequently anathema to much of the Communist left, which demanded a didactic art the masses could immediately apprehend. But such defenses of modern art had little impact on its more irrational opponents; the *New York Times* continued to receive letters frequently so "violent in their phraseology" that some were unprintable.⁴⁷

Why Dondero, who had no training in art or art criticism, should have become the defender of these unhappy traditionalists is unclear. As a congressman, however, he had proved no friend of change; it was a measure of his legislative career that he would oppose Roosevelt's domestic and foreign policy with a rigor and rhetoric that earned him in 1942, after ten years in the House, a place on *The Nation's* list of "Our Worst Congressmen"—a group distinguished not only by their "bitterness to the New Deal" but by their "essentially undemocratic outlook."⁴⁸ Unscathed by this expression of liberal

⁴⁷ *New York Times*, Sept. 6, 1950; "Boston Goes from 'Modern' to 'Contemporary'," *College Art Journal*, 7 (1948):230; "Contemporary Documents: American Sculpture 1951," *College Art Journal*, 11 (1952):280-89; Dale Nichols to Harry S. Truman, Dec. 15, 1950, Nichols File, National Collection of Fine Arts (Washington, D.C.); *New York Times*, May 22, 1953; Feb. 26, 1949. For Biddle's thinking on the issue, see his "Modern Art and Muddled Thinking," *Atlantic*, 180 (1947):58-61. Aline B. Louchheim, "'Modern Art': Attack and Defense," *New York Times*, Dec. 26, 1948, sec. 2; Howard Devree, "Modernism Under Fire," *New York Times*, Sept. 11, 1949, sec. 2; Alfred H. Barr, Jr., "Is Modern Art Communist?" *New York Times Magazine* (Dec. 14, 1952), 22-23, 28-30; Howard Devree, "State of the Art World," *New York Times*, Sept. 17, 1950, sec. 2.

⁴⁸ Will Chasan, "Keep Them Out! Our Worst Congressmen," *The Nation*, 155 (1942):438-9. Dondero's connection with Father Charles Coughlin was of interest since both lived in Royal Oak. Chasan pointed out that Dondero had inserted material from *Social Justice* in the *Congressional Record* and had in turn been quoted in Coughlin's paper. More important, however, was the close friendship he developed with J. Edgar Hoover in the mid- to late forties. See Carey McWilliams, "The White House Under Surveillance," *The Nation*, 174 (1952):150-1.

opposition, the Michigan Republican had held fast to his Congressional seat, emerging in the Truman years as an outspoken foe of communism well before he launched his first assault upon abstract artists as agents of the Kremlin. Whatever the element of political opportunism in such attacks, they reflected an intense aversion to nonobjective painting. Although Dondero explained to art critic Emily Genauer that he opposed modern art as "Communist" because it bred "dissatisfaction" by virtue of the fact that it was unintelligible to ordinary Americans whose "beautiful country" it failed to "glorify," he also emphasized its distortion, grotesqueness, and meaninglessness, adding privately, "modern art is a term that is nauseating to me."⁴⁹ This reaction, moreover, was hardly unique if the thousands of letters that eventually poured into his office are an adequate measure of support.⁵⁰

Precisely who Dondero's ideological constituents were is difficult to determine, but they extended far beyond the geographical confines of Michigan's Seventeenth Congressional District and the modest homes of Royal Oak. Dondero himself believed that he had become the spokesman for great numbers of Americans throughout the country who applauded his attacks on modern art as Communist conspiracy. After 1949, few would deny that he had become the single most important congressional "authority" on communism in art.⁵¹ Moreover, he played that role with the full cooperation of those politically conservative traditionalists who secretly plied him with damaging information about fellow artists,⁵² reinforcing in the process the popular identification of modernism with the left.⁵³

The potential strength of this alliance of esthetically and politically con-

⁴⁹ Emily Genauer, "Still Life with Red Herring," *Harper's Magazine*, 199 (1949):89; Dondero to Charles E. Plant, June 29, Nov. 19, 1956; Dondero to Elizabeth Staples, Feb. 27, 1959, Dondero Papers.

⁵⁰ Dondero asserted that he had communications from roughly 2,000 individuals and groups concerning his attack on modern art. See Dondero to Arthur B. McQueen, Jan. 7, 1957. The *Washington Evening Star* (Feb. 16, 1957) was less precise, referring simply to thousands of letters.

⁵¹ His role was accorded ample publicity upon his retirement from Congress when he was honored by such organizations as the International Fine Arts Council. In a ceremony Vice-President Nixon had agreed to attend, the gold Medal of Honor was presented to Dondero by Major General U.S. Grant, 3rd. See Helen Bassel to Dondero, Oct. 7, 1965; also "Dick" (Nixon) to "George" (Dondero), telegram, Feb. 8, 1957, Dondero Papers. A full account of the ceremony is contained in the *Cong. Rec.*, 85th Cong., 1st Sess., A1172-3. Dondero was also honored by the American Artists Professional League and the Painters and Sculptors Club of Los Angeles.

⁵² Dorothy Drew, a New York City artist who painted Dondero's portrait when he became chairman of the House Committee on Public Works, provided him with material for his speeches attacking modern art and leftist artists. See Dondero to Dorothy Drew, June 21, Aug. 6, 1957; Jan. 12, 19, 1960; also Wheeler Williams to Dondero, Aug. 27, 1965, Dondero Papers. Dondero took great care to keep this fact from becoming public lest their opponents work their "bitterness and revenge" on her. See Dondero to Drew, Feb. 5, 1964, Dondero Papers.

⁵³ While self-consciously apolitical in the late forties and the fifties, the abstract expressionists had, like most artists in New York in the thirties, been involved in leftist activities of one kind or another. See Harold Rosenberg, *The Anxious Object: Art Today and its Audience* (New York, 1966), 262. Edward Laning recalls a visit to the 8th Street Club, a favorite haunt of the abstract expressionists, where he discovered his former WPA colleagues complaining about representational painting and noted that "Nelson Rockefeller's Museum of Modern Art took up where the John Reed Club left off." See Edward Laning, "The New Deal Mural Projects" in *The New Deal Art Projects: An Anthology of Memoirs*, edited by Francis V. O'Connor (Washington, D.C., 1972), 112. This association of political and esthetic radicalism is explored by Donald D. Egbert, *Socialism and American Art* (Princeton, 1967), and *Social Radicalism and the Arts: Western Europe* (New York, 1970). For the disastrous consequences of such linkage in Germany, see Barbara Miller Lane, *Architecture and Politics in Germany, 1918-1945* (Cambridge, Mass., 1968).

servative artists and anti-Communist crusaders had been demonstrated as early as 1947, when the State Department purchased a collection of modern art for an exhibition called "Advancing American Art," to be circulated in Europe and Latin America. Although little attempt was made to recognize the struggling abstract expressionists of what would later become the New York School, the collection did include works by such established modernists as Marsden Hartley and Stuart Davis, along with other well-known artists of the period. Recognizing that these seventy-nine paintings had been accumulated within the limitations imposed by an exceedingly modest budget—only \$49,000—most critics agreed with Lloyd Goodrich of the Whitney, who, with few qualifications, called the collection a "remarkably fine one."⁵⁴ But not so the American Artists Professional League, an organization of "modern classicists" who deplored the increasing strength of "revolutionaries" "debauching" all that was "noble in art." In a letter to Secretary of State James F. Byrnes, they complained that this "one-sided collection" was tainted with radical European trends "not indigenous to our soil." Similar charges came from other artist groups such as the National Academy of Design, Allied Artists, the Salmagundi Club, and the Society of Illustrators.

Echoing their objections, the Hearst press was especially vitriolic in its castigation of this latest federal "folly." While the Baltimore *American* complained about the inclusion of works by "left-wing painters who are members of Red Fascist organizations," the New York *Journal American* reserved its animus for the paintings themselves, which were described as "incomprehensible, ugly and absurd," a "lunatic's delight." *Look* magazine joined the fray, as did conservative radio commentator Fulton Lewis, Jr.⁵⁵

Governmental critics were similarly outraged. Referring to this "sinister" exhibit of modernists, Illinois' Fred Busbey deplored the "weird," "unnatural" depiction of human forms. Georgia's Edward Cox (Democrat) agreed that no sane man could have painted such "crazy" pictures, while Mississippi's John Rankin (Democrat) suggested that they were "Communist caricatures . . . sent out to mislead the rest of the world as to what America is like."⁵⁶ The president agreed, at least on the esthetic merits of the exhibit; of a Yasuo Kuniyoshi painting of a circus scene, Truman remarked that "if that's art, I'm a Hottentot."⁵⁷

⁵⁴ A complete listing of the paintings is contained in the *Cong. Rec.*, 80th Cong., 1st Sess. (1947), 5225. For critical opinions, see John D. Morse to William Benton, Oct. 10, 1946, Hudson Walker Papers; Alfred M. Frankfurter, "American Art Abroad: The State Department's Collection," *Art News*, 45 (1946):21-30ff.; and Goodrich interview. Edward Alden Jewell, of the *New York Times*, predictably dissented. His view of the exhibit is of interest because of the tendency of conservatives to link modern art to the political left. See "Eyes to the Left," *New York Times*, Oct. 6, 1946, sec. 2.

⁵⁵ "League Protests to the Department of State," *Art Digest*, 21 (1946):32-33; Ralph M. Pearson, "Hearst, the A.A.P.L. and Life," *Art Digest*, 21 (1946):25; Peyton Boswell, "Humor on the Right," *Art Digest*, 21 (1946):3; "Government Tries Again," *Art Digest*, 22 (1947):32-33.

⁵⁶ Hearings before the Subcommittee of the House Committee on Appropriations, *Department of State Appropriation Bill for 1948*, 80th Cong., 1st Sess. (Washington, 1947), 412 ff., and *Cong. Rec.*, 80th Cong., 1st Sess. (1947), 5187-5292.

⁵⁷ As Robb observed, the Kuniyoshi *Circus Rider* that drew Truman's fire was not one of the more abstract paintings in the collection. See "Art News from Chicago," 39.

Not surprisingly, the State Department, riddled with charges of subversion and under severe scrutiny from congressional appropriation committees, capitulated, cancelling the exhibition, despite the protests of New York artists, museum officials, dealers, and art journal editors, who deplored the incident as a serious threat to artistic freedom.⁵⁸ With the future of the entire Office of International Information and Culture Affairs in doubt, Secretary of State George C. Marshall announced that there would be “no more taxpayers’ money for modern art.”⁵⁹ The USIA eventually followed suit. Arguing that the agency was interested in art exhibitions only in so far as they provided “a means of interpreting American culture to other peoples,” its spokesman insisted that the government “should not sponsor examples of our creative energy which are nonrepresentational.” Emphatically, he added: “We are not interested in purely experimental art.”⁶⁰ The perception of *avant-garde* art as un-American had now been incorporated into official policy.

Although government officials subsequently modified their stand, hostility to modern art had by no means entirely dissipated when the USIA began plans to send abroad paintings and statuary for the American National Exhibition that Vice-President Richard M. Nixon was to open in Moscow in the summer of 1959. With talk now of detente with Russia, the agency relaxed sufficiently to include in the four-man jury charged with assembling the exhibit Lloyd Goodrich, Director of the Whitney and a persistent proponent of artistic freedom. Charged with the task of selecting works that would reflect the vitality and variety of American art over the previous forty years, Goodrich and his colleagues naturally included examples of the early modernists and those of the New York School along with social realists, American scene painters, and a variety of others.⁶¹ Anticipating objections, they sought to outmaneuver potential critics by withholding any announcement of the works selected until they were packed for shipping, thus presenting opponents with a kind of *fait accompli*.⁶²

⁵⁸ New York *Times*, May 6, 1947; also editorial comment in *Art News*, 46 (1947):13 and *Art Digest*, 22 (1947):7, as well as the May 1947 issue of the Artists Equity *Newsletter*. The collection, after being summarily recalled from abroad, was consigned for disposal as war surplus; Lloyd Goodrich arranged for a showing at the Whitney, however, so that prospective buyers from public institutions might see the paintings before purchasing them. Thus they happily escaped the fate of those WPA oils that were sold by the government for the price of the canvas. See Aline B. Louchheim, “The Government and Our Art Abroad,” New York *Times*, May 23, 1948, sec. 2. Even this action was criticized by Representative Busbey. See *Cong. Rec.*, 80th Cong., 2nd Sess. (1948), A4115-6.

⁵⁹ New York *Times*, May 6, 1947.

⁶⁰ A. H. Berding’s statement of USIA policy was enunciated at a meeting of the AFA in October 1953. Although not stated in his original speech, a second form of censorship was made public in an article in the Washington *Post and Times Herald* on March 6, 1955, quoting agency policy on the political associations of artists. The USIA refused to exhibit “works of avowed Communists, persons convicted of crimes involving a threat to the security of the United States, or persons who publicly refuse to answer questions of congressional committees regarding connection with the Communist Movement.” As Alfred Barr (“Letters to the Editor”) pointed out, this 1955 statement actually represented a relaxation of censorship as previously practiced by federal agencies that had banned or tried to ban the work of artists obviously less “subversive” than those who were “avowed Communists” or who had taken the Fifth Amendment. See “Artistic Freedom,” *College Art Journal*, 15 (1956):184-89.

⁶¹ For the complete listing of works initially included in the exhibit, the rationale behind their selection, and Senator Philip Hart’s defense of the exhibit, see *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 9812-14.

⁶² Goodrich interview.

Four days after the list was released the sniping began.⁶³ Naturally, Jackson Pollock's *Cathedral* was criticized, as were other "meaningless abstractions."⁶⁴ Wheeler Williams, appearing before the House Un-American Activities Committee, which was investigating several of the artists involved, called the painting a "childish doodle." Fortunately, President Eisenhower declined to act as censor even though he was far more unhappy with Jack Levine's unflattering depiction of a returning general in *Welcome Home*. When asked about the exhibit at a news conference, the president admitted that his favorite was an Andrew Wyeth painting. He went on to say that perhaps future selection committees might include "one or two people that, like most of us here . . . are not too certain exactly what art is, . . . but know what we like and what America likes." "What America likes," he insisted, "is after all some of the things that might be shown."⁶⁵ Although it was subsequently decided to send additional traditional works to Moscow, Jack Levine's *Welcome Home*, with its social commentary, and Jackson Pollock's *Cathedral* remained intact. For champions of artistic freedom, Eisenhower's decision, combined with the extensive criticism they were able to marshal against the investigation of the House Un-American Activities Committee, represented a major breakthrough.⁶⁶

Although there would be future cancellations, the old relish for congressional intervention had clearly begun to wane. The USIA, fearful that more subtle pressure would be exerted through budget cuts, remained exceedingly wary through the early sixties, preferring exhibits that produced little publicity and no controversy. Gathering courage, the agency finally ventured into those international competitions, the biennials, but the news that an American artist had won the prize at the São Paulo Bienal in 1963 and again the following year at the Venice Biennale gave USIA officials an acute case of the jitters. The publicity was considerable, and with the top prize going in 1964 to Robert Rauschenberg for one of his painterly photographic collages, the agency was sure that budgetary repercussions would be forthcoming. Although their predictions were not fulfilled, the fears persisted until 1965, when the International Exhibits were moved out of the USIA and into the Smithsonian.⁶⁷

⁶³ Representative Francis E. Walter's attack on the exhibit and his charges that thirty-four of the sixty-seven artists represented had affiliations with Communist causes are contained in the *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 9746-48. For testimony before Walter and his House Un-American Activities Committee, see HUAC Hearings, 899-963.

⁶⁴ For the discussion of the Pollock painting, see HUAC Hearings, 915.

⁶⁵ A complete transcript of Eisenhower's press conference is contained in the *New York Times*, July 2, 1959. Jacob Javits made a far more eloquent speech on behalf of artistic freedom in the Senate where he argued that whether or not one like modern art it was closely identified with the American scene and should not be removed from the exhibit. "To send . . . officially dictated art," he insisted, "will only be to parrot the performance of their own government." *Cong. Rec.*, 86th Cong., 1st Sess. (1959), 12548-9. Levine, incidentally, specifically excluded Eisenhower and Omar Bradley from attack, professing great respect for both generals, but not for the military establishment generally.

⁶⁶ Interview with Lois Bingham, Feb. 17, 1969. Bingham, Chief of the Office for Exhibitions Abroad at the National Collection of Fine Arts, worked in that area during the fifties when International Exhibits were under USIA auspices.

⁶⁷ *Ibid.*

By that time it was apparent that Dondero and his traditionalist allies had been fighting a rear-guard action in their battle against modernism. The esthetic leadership of these artists long since eroded, they ultimately lost political influence as well. Even their temporary victories proved hollow ones, for, in the final analysis, they had been outmaneuvered by more sophisticated individuals eager to capitalize on the fact that *avant-garde* art and culture exist only in a society that is liberal-democratic (politically) and bourgeois-capitalist (socioeconomically).⁶⁸ Thwarted by repeated interference in USIA and State Department exhibits throughout the fifties, more enlightened anti-Communists, among them Nelson Rockefeller, had naturally turned to “mother’s museum” and its newly expanded international program.⁶⁹ Accordingly, the Museum of Modern Art arranged for U.S. participation in a variety of international exhibits, putting together in the process a major show of abstract expressionist art for circulation in Europe in 1958–59. Entitled “New American Painting,” the exhibition included a catalog with a comprehensive introduction by Alfred Barr, former Director of the Museum of Modern Art and a long-time advocate of the abstract expressionists. In an essay that subtly reflects the manner in which this self-consciously apolitical vanguard was becoming identified with the cause of human freedom, their work used as a form of “benevolent propaganda for foreign intelligentsia,”⁷⁰ Barr wrote: “Indeed one often hears Existentialist echoes in their words, but their ‘anxiety,’ their commitment, their ‘dreadful freedom’ concern their work primarily. They defiantly reject the conventional values of the society which surrounds them, but they are not politically engaged even though their paintings have been praised and condemned as symbolic demonstrations of freedom in a world in which freedom connotes a political attitude.”⁷¹

For Dondero and his allies there was irony indeed in that, as New York replaced Paris as the capital of the art world, the painting that earned for this nation unparalleled pre-eminence and prestige was the art of the abstract expressionists. And there was greater irony still in that, through the transmuting context of the Cold War, this new generation of New York painters ultimately came to be regarded as the embodiment of the kind of freedom denied their colleagues behind the iron curtain, their works celebrated as quintessentially American.⁷² So rapid and complete was this identification that by the mid-sixties modern art itself had somehow become inextricably linked with the United States as if only in America could the *avant-garde* “spirit” truly flourish. Governmental agencies could at last join private museums and dealers in sending abroad modernist evidence of our creative coming of age—and they did so with little fear of interference.

⁶⁸ Renato Poggioli has thoughtfully explored the political conditions under which *avant-garde* culture can flourish in *The Theory of the Avant-Garde* (Cambridge, Mass., 1968), 95–109.

⁶⁹ For the role of the Rockefellers and the Museum of Modern Art, see Eva Cockcroft, “Abstract Expressionism, Weapon of the Cold War,” *Artforum*, 12 (1974):39–41.

⁷⁰ The phrase was used by Max Kozloff in his suggestive “American Painting During the Cold War,” *Artforum*, 11 (1973):43–54.

⁷¹ Quoted in Cockcroft, “Abstract Expressionism,” 41.

⁷² Kozloff, “American Painting During the Cold War.”

That such fears should have persisted as long as they did among intelligent public servants, however, is evidence of the zeal and commitment of people who were determined to keep American art recognizably "American." If they could not control the power of a militant Russia, then they would at least impose an ideological and esthetic conformity associated with their standard of Americanism on art and artists receiving any form of public patronage. A coalition of esthetically and politically conservative legislators and artists, veterans and patriotic groups, they were always a minority. But the events of the McCarthy era demonstrated with frightening force how much political leverage can be gained from the animosities and passions of a small, but articulate and effective minority. Deeply disturbed by the international domestic tensions that beset Cold War America, they sought security from without by searching for the enemy within. Scrutinizing the whole apparatus of government, education, religion, and the communications media, they focused on the "front" affiliations of many of this nation's leading artists. What they saw was evidence not of youthful dedication to Marxist ideology or even a persisting affiliation with the Communist Party, but of contemptible collusion with a conspiratorial power that had reduced half of the world to chains. And when they examined the paintings themselves, they found in the works of socially conscious artists a commentary on society and politics that seemed to suggest not so much a personal protest against injustice as visual evidence of disloyalty. As individuals who sought security in a unified nation, they were unable to tolerate the evidence of social cleavage such paintings reflected. A Marxist concept—class conflict—seemed not only patently un-American but personally threatening.

Convinced that national unity and domestic security were jeopardized by art with "subversive" social commentary and artists with "front" associations, such people possessed little tolerance for new esthetic forms and values. The rejection of modern art and the argument that it was a weapon in the Communist arsenal, however strained the logic, must be carefully scrutinized for what it reveals about the psychological character of antiradicalism as well as the symbolic significance of public patronage.

The identification of modern art with foreign influence, like the identification of political and cultural radicalism, is of long standing. When exhibitors displayed hundreds of postimpressionists, fauve, and cubist works before scandalized Americans at the Armory Show in 1913, some leading art critics had been quick to detect a foreign conspiracy in the invasion of "Ellis Island art," as one commentator termed the new styles.⁷³ Ridiculed by culturally sanctioned artists and critics, a small *avant-garde* community struggled in subsequent years to create what it conceived to be an indigenous American art that would reflect contemporary life, but such efforts met with scant

⁷³ Barbara Rose, *American Painting Since 1900: A Critical History* (New York, 1967), 13. The persistent charge that modern art was "foreign" is evident in the response to the awarding of the first prize of the Chicago Art Institute to abstract expressionist William Baziotes in 1947. Wrote C. J. Bulliet of the *Daily News*: "These 'isms' grown stale and sterile in the land of their origin, are further enfeebled crossing the Atlantic." Bulliet's comment is quoted in Dore Ashton, *The New York School: A Cultural Reckoning* (New York, 1973), 147.

acceptance from the larger society. It was a measure of public intransigence to the new art that in the 1920s, Max Weber, one of the greatest American cubist painters, was so impoverished that he lacked money for paint and canvas.⁷⁴ A majority of Americans, literal-minded in their taste, continued to assume that American art must be representational.⁷⁵ Never had this identification been more firmly established than in the thirties when many artists, whether rurally oriented regionalists or the more radical urban social realists, sought to detach themselves from "foreign influence" and immerse themselves in the "spirit of the land," capturing the texture of reality in a movement appropriately known as the "American Scene."⁷⁶ Like their counterparts in fiction, theater, and dance, they sought in a new genre of actuality to record and clarify the American experience in explicit, commonly recognizable images that would make possible immediate identification between picture and audience, artist and public.⁷⁷ This effort to capture reality by making art a vehicle for realistically recording places, events, and things, rather than exploring the vast inner landscape of the psyche, coincided, moreover, with a passionate desire of esthetic nationalists for an authentically American art that would rival the great national schools of Europe. It was a significant coincidence and one that would further strengthen the belief that American art was representational art—recognizably American in content, non-European (non-abstract) in style, and democratic in accessibility.⁷⁸ Such had been the case in the thirties and forties, and such would be the case in the fifties.

Traditionalists had a stake in seeing that this formula persisted. The hostility to modernism on the part of many traditionalist artists and sculptors had its origins in many sources, but most noticeably economic rivalry and status anxiety. Academic sculptors in organizations such as the National Sculpture Society had long enjoyed a lucrative monopoly on official art. As suppliers of statuary to public buildings and war memorials, they were responsible for those huge Victorias, Columbias, Giants of Trade and Industry, to say nothing of the muscle-bound horses, that abound in state capitals as well as Washington. Conceived in the classic pattern, it was precisely what was calculated to win approval from Washington's Fine Arts Commission.⁷⁹

Though an attachment to recognizable subject matter persisted through the forties and fifties, abstract expressionist or "action" paintings were exhibited in Peggy Guggenheim's gallery, *The Art of This Century*, as early

⁷⁴ Rudi Blesh, *Modern Art U.S.A.: Men, Rebellion, Conquest, 1900-1956* (New York, 1956), 85-86.

⁷⁵ Lionel Trilling noted a related tendency in American literature wherein liberal intellectuals have followed the Parrington tradition of associating "reality" with democracy. See his "Reality in America" in *The Liberal Imagination: Essays on Literature and Society* (New York, 1957), 1-19.

⁷⁶ Matthew Baigell, *The American Scene: American Paintings of the 1930's* (New York, 1974), 18.

⁷⁷ *Ibid.* See also William Stott, *Documentary Expression and Thirties America* (New York, 1973).

⁷⁸ See my "Art and the People: The New Deal Quest for a Cultural Democracy," *Journal of American History*, 62 (1975): 334 ff; and Baigell, *The American Scene*, 13, *passim*, 45. That so much of the New Deal art was "American Scene" in effect gave it an official imprimatur as Baigell has rightly observed.

⁷⁹ Charlotte Devree, "Is This Statuary Worth More Than a Million of Your Money?" *Art News*, 54 (1955): 34-37ff.

as 1943. By 1945, when Jackson Pollock held his second one-man show, the Wyoming-born artist again received enthusiastic endorsement from both James Johnson Sweeney and Clement Greenberg, the latter critic praising him as the “strongest painter of this generation, perhaps the greatest one to appear since Miró.” By the mid- to late-forties, abstract expressionists were lauded not only in *Partisan Review*, *The Nation*, *Magazine of Art*, and *Art News*, but also in popular magazines such as *Life*. Critical acclaim was naturally followed by museum purchases. The Museum of Modern Art, which bought Pollock’s *She Wolf* as early as 1944, picked up paintings by Arshile Gorky and Robert Motherwell two years later. In 1947, a William Baziotes canvas received the major prize at the show of the Chicago Art Institute following its exhibit at the Venice Biennale. Moreover, as the fifties progressed, abstract expressionist painting was bought, shown, lectured on, and talked about by an articulate and vocal generation of artists and critics in more and more museums, colleges, art centers, and magazines. Thus, behind the aggrieved cry of dispossessed traditionalists in the conservative art organizations was the impending reality of vanguard triumph. Understandably threatened economically and stylistically, they made common cause with other Americans to whom modernism in any guise was perceived as threatening.⁸⁰

Although economic rivalry and partisan politics underlay much of the hostility to modern art, it would be a mistake to underestimate the intense hatred of abstraction that permeated the attacks of congressmen such as Dondero and the letter writers who provided them unsolicited support. Clearly, these people were threatened at a very basic level. And the reason why lies not only in their world view but also in the intrinsic characteristics of modern art and most especially abstract expressionism.

The abstract expressionists of the New York School were genuine esthetic revolutionaries—but revolutionaries who were steeped in the modern tradition. Having mastered the esthetic basis of modernism with the help of Hofmann, Albers, Mondrian, and the many surrealists who fled Europe during the war, they rejected existing realistic and geometric tendencies while borrowing the biomorphic shapes, mythic symbols, and automatist technique of the surrealists. The result was not merely an eclectic synthesis of European sources but a highly original style that quite literally extended the frontiers of art. Although landscape and figure elements can be discovered in many works, readily identifiable subject matter was rejected. Turning instead to private visions, insights, and most especially the subconscious, the abstract expressionists plumbed the depths of their own experience for metaphors and symbols that would somehow possess universal meaning. Content thus consisted of ambiguous forms with loosely defined, perhaps even subliminal, multivalent associations arrived at not through prior determination but in the

⁸⁰ Irving Sandler, *The Triumph of American Painting: A History of Abstract Expressionism* (New York, 1970), 79, 211–12; Rosenberg, *Anxious Object*, 77. It is significant also that Pollock, de Kooning, and Gorky were included in the American exhibit at the 1950 Venice Biennale.

actual process of painting. Gone were the pleasures of easy recognition and the enjoyment of technical dexterity in the imitation of material form and surface so evident in the painting of Andrew Wyeth. Appreciation of abstract expressionism, critic Harold Rosenberg noted, now required a consciousness of history since the art historical reference, especially in “over-all” abstraction, was often the only content. “The old pleasure of seeing ‘life’ inside the frame must be augmented, if not replaced,” Rosenberg noted, “by the stimulation of recognizing an inspired side glance to, say ‘Les Demoiselles d’Avignon.’”⁸¹

Thus for the untutored viewer, the art of the abstract expressionists was not only subjectless in the traditional sense, but it seemed conspicuously foreign in terms of historical antecedents. And since public consciousness had no part in the work, it was perforce elitist.⁸² While this characteristic was widely accepted by abstract expressionists self-consciously alienated from the middle class, it was anathema to frustrated viewers whose very bafflement reminded them that esthetically they had not yet arrived after all—and, indeed, might never make it. Fed the predigested pap of mass culture, they were simply unprepared for the sheer effort required in the process of visual analysis.⁸³ Opposition, as André Malraux had predicted of any mass public confronting *avant-garde* art, derived not only from ignorance but from an obscure sense of “betrayal.”⁸⁴ Thus rejected and rejecting, many people were no doubt further enraged by the implications of it all: the notion of a hierarchy of taste—the possibility of cultural classes in a democratic state.⁸⁵ And there were still other barriers. The art of the abstract expressionists was also perceived as functionless when function was a quality that Americans had traditionally demanded in the visual arts. Indeed their very legitimization in this country had been accomplished through the attribution of didactic qualities. And perhaps worse still, these canvases with their static masses of color or tangled surfaces of dripped paint provided no indication of the artist’s talent or index to his actual labor. Unlike an Andrew Wyeth painting with its photographic rendering of each blade of grass, a Mark Rothko or a Jackson Pollock communicated no evidence of hard work. And as one art critic has noted, respect for hard work amounts to an “esthetic prejudice” in America.⁸⁶

The elitist character of abstract expressionism, its nonrepresentational

⁸¹ Rose, *American Painting Since 1900*, 61, 66, 70–73, 89; Sandler, *Triumph of American Painting*, 1, 3, 29, 62, 92–93; Rosenberg, *Anxious Object*, 77.

⁸² Gottlieb put the matter succinctly: “The modern artist does not paint in relation to public needs or social needs—he paints only in relation to his own needs.” See Adolph Gottlieb, “The Artist and Society,” *College Art Journal*, 14 (1955):99.

⁸³ The extent to which mass culture has rendered the general public less able to deal with high culture, especially modern art, is explored in a variety of essays in *Mass Culture: The Popular Arts in America*, eds. Bernard Rosenberg and David Manning White (Glencoe, 1958). See especially, Irving Howe, “Notes on Mass Culture,” 499.

⁸⁴ Quoted in Poggioli, *The Theory of the Avant-Garde*, 180.

⁸⁵ The hostile response of the “middlebrow” to the possibility of cultural classes in a democratic state is suggested by Leslie Fiedler in “The Middle Against Both Ends,” in *Mass Culture*, eds. Rosenberg and White, 547.

⁸⁶ Rose, *American Painting Since 1900*, 43.

quality, its perceived lack of technical dexterity of function and even of meaning are thus but a few of the factors that would understandably make the art of the New York School inaccessible to many, if not indeed most, Americans in the late forties and fifties. But the vast majority of individuals who found such art unintelligible or unattractive might dismiss it simply as “junk”; they would not respond by calling it decadent, degenerate stuff that was communist inspired. To understand why some Americans would respond in this fashion and do so with an intensity and passion that on occasion rendered their attacks unprintable requires further examination of the art itself and its psychological impact.

These were people, it must be remembered, who were already fearful that traditional American virtues had been undermined by cosmopolitans and intellectuals, the old competitive capitalism gradually eroded by Socialist and Communist schemes, and national security compromised by treasonous plots. To compound matters, they were confronted in abstract expressionism by a form of vanguard art characterized by two dimensionality, fluid space, lack of closed shapes, a deliberately unfinished quality, and an “overall” composition that diffused any notion of focus. Complex, cosmopolitan, and ever-changing, it was intrinsically at odds with the need for certitude and control. Control, moreover, was precisely what was threatened by the strong Freudian elements in abstract expressionism. *Avant-garde* American painters, as has been previously noted, borrowed heavily from the surrealists, emphasizing the unconscious as a source of imagery and automatism as a technique whereby painting became spontaneous action—a gesture of “liberation from value—political, esthetic, moral.”⁸⁷ Reality in art now embraced not only the outer, objective world of nature and human activity, but also the inner, subjective, psychological world of self. Sophisticated critics such as Ben Shahn could recognize that this inner reality offered a “rich, almost limitless panorama”—an unparalleled source of images and symbols—while simultaneously deploring the apparent abdication of intention on the part of the artist.⁸⁸

For some of the less sophisticated, the free play of the unconscious could mean only irrationality in its most pejorative sense—the beast within. In a pamphlet significantly entitled *The Animal Stalks*, a realist artist argued that “the insane and sensual animal aspects of ‘Modern’ art must be as surely repressed as are obscene photographs and narcotics . . . and an art that ‘feeds’ the mind encouraged [lest] the Western World . . . perish.”⁸⁹ Although the author would doubtless have resented the comparison, his were sentiments worthy of that notorious Stalinist censor and suppressor of creative thought,

⁸⁷ Harold Rosenberg, *The Tradition of the New* (New York, 1959), 30.

⁸⁸ Ben Shahn, *The Shape of Content* (Cambridge, Mass., 1957), 43ff. In these Charles Eliot Norton lectures at Harvard, Shahn provides a lucid and perceptive critique of abstract expressionism that belies the notion that such opposition issues only from the political right. His distress at the abdication of intention is shared by Jacques Barzun, who suggests that the progressive elimination of purpose culminating finally in aleatory art has made it necessary to refer to older genres as “intentional art.” See Barzun, *The Use and Abuse of Art* (Princeton, 1974), 55.

⁸⁹ Dale Nichols, *The Animal Stalks* (n.p., 1953), 8. The pamphlet is available in the Nichols File, National Collection of Fine Arts.

Andrei Zhdanov, whose own efforts to stifle modernism in the USSR reflect what one scholar has described as an almost pathological tendency toward repression of the sexual element in art.⁹⁰ Like their Soviet counterparts, these American opponents of modernism never underestimated the power of the artist to coerce the senses. Thus, when they lambasted abstract expressionism, they did so out of a compelling, if little understood, need to maintain a rigid view of reality characteristic of what might be called the "cultural fundamentalist"⁹¹—a view wherein the irrational was repressed, causation simplified, change controlled, heterogeneity denied, loyalty affirmed, national unity and personal esteem preserved. Precisely how modernism in art, as in other aspects of life, threatened that reality was never fully and satisfactorily delineated by its opponents, but they correctly perceived that it did. The problem, then, was not just one of ignorance but of innate antipathy or, as Renato Poggioli would say, not of "exegesis but of psychology" for, in the final analysis, the perception of art is essentially a psychophysiological process.⁹²

The intensity of opposition becomes more understandable if it is recognized that public patronage, like Prohibition, was a cultural issue fraught with symbolic significance.⁹³ The fact that the USIA and State Department were sending abroad modern art under official auspices raised the question whether the power and the prestige of the government had been placed now on the side of the modernists. Inasmuch as abstract expressionism symbolized a whole set of cultural values associated with modernism, such patronage served to legitimate those values—to affirm their validity while simultaneously discrediting those associated with cultural fundamentalism. Thus in the contest for social dominance, many traditionalists no longer enjoyed the symbolic satisfaction of being exclusively identified with publicly affirmed values and norms. Confronted with this evidence of declining hegemony and control, some responded with the self-righteous passion of the dispossessed.

That they also responded with charges of communism is hardly surprising

⁹⁰ Maynard Solomon, ed., *Marxism and Art* (New York, 1973), 238.

⁹¹ The term "cultural fundamentalist" has reference to "culture" in the anthropological sense rather than as a synonym for the fine arts and to "fundamentalist" in a nonreligious as well as a religious context. The term is not meant to be perjorative. My conception of the term has been influenced by a number of studies, among them: Theodore W. Adorno et al., *The Authoritarian Personality* (New York, 1950); Richard Christie and Marie Jahoda, eds., *Studies in the Scope and Method of the Authoritarian Personality* (Glencoe, Ill., 1954); M. Brewster Smith, Jerome S. Bruner, and Robert W. White, *Opinions and Personality* (New York, 1956); Neil J. Smelser, *The Theory of Collective Behavior* (New York, 1962); and Eric Hoffer's idiosyncratic but provocative *The True Believer* (New York, 1958).

⁹² Poggioli, *The Theory of the Avant-Garde*, 154. Arnold Hauser has noted in *The Philosophy of Art* that the mass public reacts "not to what is artistically good or bad but to features that have a reassuring or disturbing effect upon their course of life; they are ready to accept what is artistically valuable provided that it supplies vital value for them by portraying their wishes, their fantasies, their day-dreams, provided that it calms their anxieties and increases their sense of security." Quoted in Berel Lang and Forest Williams, eds., *Marxism and Art: Writings in Aesthetics and Criticism* (New York, 1972), 274. For an introduction to the psychology of visual perception, see the writings of Rudolf Arnheim, especially *Visual Thinking* (Berkeley, 1969). There is extensive literature documenting the ways in which perceptual behavior is affected by personality. See, for example, Jerome S. Bruner and Cecile C. Goodman, "Value and Need as Organizing Factors in Perception," *Journal of Abnormal Psychology*, 42 (1947):33-44.

⁹³ The analogy between Prohibition and public patronage was suggested by the reading of Joseph Gusfield's *Symbolic Crusade: Status Politics and the American Temperance Movement* (Urbana, Ill., 1963).



Jack Levine, "Welcome Home" (1946). Courtesy of The Brooklyn Museum, J. B. Woodward Memorial Fund.

when we recognize that communism itself had become for many a symbolic issue that had less to do with a foreign ideology or even the realities of international politics than with the forces of change. For those who responded to the rhetoric of a Joseph R. McCarthy or George A. Dondero, anti-Communism became a call not only for political conservatism but for competitive capitalism, a nativistic nationalism, and religious orthodoxy. Anti-statist, antibureaucratic, antforeign, and anti-intellectual, it was also anti-modernist. And in its gallery of "enemies," the Museum of Modern Art was as suspect as the National Council of Churches. Indeed, so dominant is this preservatist character that our understanding of an era may well be enhanced if we view the anti-Communist crusade of the fifties and early sixties not just as a political response to legitimate and highly disturbing Cold War problems but also as a revitalization movement designed to eliminate foreign influences and revive traditional values and beliefs in a period of societal stress.⁹⁴ In the process of revitalization, countersubversives focused, as they did in the wake of World War I, on the familiar readily recognizable symbols of Americanism, emphasizing always the concrete, the definite, the unambiguous. And these, of course, were the very qualities that were antithetical to the modern art of the abstract expressionists, with its ambiguous forms, fluid space, open shapes, and multivalent associations. Thus it is hardly surprising that modern art became emblematic of the forces threatening the psychic equilibrium of the anti-Communist right.

⁹⁴ The most effective treatment of an anti-Communist crusade in terms of a revitalization movement is Stanley Coben's "A Study in Nativism: The American Red Scare of 1919-1920," *Political Science Quarterly*, 79 (1964): 52-57. Gregory Guroff of Grinnell College has called my attention to a comparable response in the Soviet Union where real international responsibilities acquired in the years following World War II seemed to engender a similar rejection of cosmopolitan culture and a furiously nationalistic campaign complete with renewed denunciations of nonrepresentational art, atonal music, and nonsocialist realist literature.

Consensus or Conflict: The Dilemma of Islamic Historians

A Review Article by ANDREW C. HESS

MARSHALL G. S. HODGSON. *The Venture of Islam: Conscience and History in a World Civilization*. Volume 1, *The Classical Age of Islam*; volume 2, *The Expansion of Islam in the Middle Periods*; volume 3, *The Gunpowder Empires and Modern Times*. Chicago: University of Chicago Press. 1974. Pp. xii, 532; vii, 609; vi, 469. \$20.00 each.

MAURICE LOMBARD. *The Golden Age of Islam*. Translated by JOAN SPENCER. (North-Holland Medieval Translations, volume 2.) Amsterdam: North-Holland Publishing Company; distrib. by American Elsevier Publishing Company, New York. 1975. Pp. x, 259. Cloth \$20.95, paper \$14.75.

V. J. PARRY and M. E. YAPP, editors. *War, Technology and Society in the Middle East*. New York: Oxford University Press. 1975. Pp. viii, 448. \$22.50.

JULIUS WELLHAUSEN. *The Religio-Political Factions in Early Islam*. Translated by R. C. OSTLE and S. M. WALZER. Edited by R. C. OSTLE. (North-Holland Medieval Translations, volume 3.) Amsterdam: North-Holland Publishing Company; distrib. by American Elsevier Publishing Company, New York. 1975. Pp. xi, 183. \$9.95.

FRANZ ROSENTHAL. *The Classical Heritage of Islam*. Translated by EMILE MARMORSTEIN and JENNY MARMORSTEIN. (The Islamic World. Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xx, 298. \$15.00.

NIKKI R. KEDDIE. *Sayyid Jamāl ad-Dīn "al-Afghānī": A Political Biography*. (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 479. \$20.00.

LENN EVAN GOODMAN, translated with introduction and notes by. *Ibn Tufayl's Hayy Ibn Yaqzān*. (Library of Classical Arabic Literature, volume 1.) New York: Twayne Publishers. 1972. Pp. ix, 246.

PHILIP K. HITTI. *Capital Cities of Arab Islam*. Minneapolis: University of Minnesota Press. 1973. Pp. v, 176. \$7.95.

TO PROCLAIM HERE THE EXISTENCE of a crisis in the field of Islamic history might not even carry the reader's interest beyond this topic sentence
This review profited greatly from criticism it received in a seminar on modernization at Temple University, March 26, 1976.

when the historical profession faces more fundamental difficulties than the changing demands of what is sometimes described as an esoteric field. Nonetheless, the writing of Islamic history in the West is at some kind of crossroads; and the news of this event ought to be broadcast if the effort to understand the past on a level that goes beyond the old division of the globe into autonomous cultural areas is to be preserved.

Before proceeding to an analysis, based on some recent publications, of the modern dilemmas of Islamic historians, an explanation ought to be developed for the role the study of this unfamiliar civilization plays in the West. One of the main justifications for expending so much energy on the exotic languages and cultures of the Middle East was that Islamic history represented such a huge chunk of human experience that it was worth-while remembering. It developed, however, that other factors were at work leading Western scholars to the subject of Islamic history. The examination of another's culture turned out to be an effective means of confirming the integrity of one's own tradition; and, in the service of various forms of imperialism, knowledge of Islamic ways was necessary if Muslims were to be convinced of the superiority of Western institutions. Whatever the motivations, the study of Islamic history soon became a dialogue between Western scholars and the culture-bearing elite of the other tradition in a manner that greatly assisted the process of understanding.

Recently, however, the lines of communication between Western and Middle Eastern historians of Islamic civilization have become fouled by the revolutionary processes of modern life. New communities of scholars—nationalist, reformist, Marxist—emerged in the Middle East to ask questions about the past that had either been avoided by former historians or simply not examined. Many of these same Middle Eastern writers further compounded the problem of communication by adopting a hostile and decidedly anti-Western tone in their work. Meanwhile, the response of Western historians to the first signs of a Middle Eastern attempt to modernize Islamic history was to carry on as before, but with a good deal of uneasiness. Some applied the discoveries of modern social science to the field. Yet the Western base upon which most of the theory rested only added to a vague sense of intellectual confusion among Islamicists as, alas, they found themselves confronted by another problem of translation: what does feudalism, or better, class, mean in a region of the world with a history so fundamentally different from that of the West?

While the reasons for the troubles that beset the writing of modern Islamic history are multifold, one of the basic causes of intellectual difficulties arises from the imperviousness of traditional Islamic history to modern criticism. There are, currently, four schools of historical activity in the Middle East. The Marxists and positivists inhabit the distant intellectual horizon. More central, hence far more popular, especially in Turkey and Iran, is national history. But this body of historical work, like much of the reformist thought in the nineteenth century, has grown up with, rather than supplanted, an older

vision of the past.¹ Most widespread, then, especially in Arab lands, are the traditional histories. Faithful to the themes of classical Islamic scholars, these works view history in terms of a divinely guided Muslim community. Not modern, they preserve the historical framework of a bygone era. Thus, if the current effort to describe the past were to be summed up, it would yield, paradoxically, both extraordinary consistency and diversity.²

To penetrate the psychological armor that the traditional vision of history erects against reality, modern authors of Islamic history must contend with three methodological problems: an aversion to applying internal criticism to the reports of responsible witnesses; the effort to emphasize the universal over the particular; and the constant desire to stress the value of consensus rather than conflict. Why these elements of an inherited paradigm are so important demands an explanation based upon Islamic history.

The first characteristic of the classical vision proceeds from the manner in which Islam was revealed to man. The Koran was God's word, not Mohammed's. Man, therefore, did not question the truthfulness of the revelations. When the Koran proved to be insufficient for the formation of a great civilization, Islamic scholars expanded the content of their culture on the basis of the testimony of believers. By extension, critical techniques were employed not to verify the content of what was reported to be Islamic practice, but to check whether or not the reporter was reliable. Even in the case of conflict between the majoritarian and oppositional versions of Islam, Sunnism and Shiism, rational criticism was used only to weaken the position of the opponent and not to change the doctrine of one's own party. In modern times one striking manifestation of the strength of this Islamic method of handling evidence is the failure of Middle Eastern scholars to apply modern literary criticism to the Koran and have the results accepted.³

A second major theme pervading all of Islamic history is the great value placed upon avoiding conflict based upon internal particularisms. Mainly a product of the struggle against an Arab definition of Islam, the drive to integrate as many groups as possible within the Community finds its internal expression in the universal histories written after the Abbasid revolution of 750. In these encyclopedic works the important division was not between Arabs, Persians, and Turks but between the House of Islam and the House of War, between the cultural region ruled by Islam and the outside world.

A third crucial element of the classical methodology comes from the importance Muslim elites attached to the achievement of consensus. On this point the history of how the Community's cultural leaders discovered the normative religious law, which is at the core of Islam, provides the best example of the consensual thrust of this great culture. One of the pillars of the legal theory

¹ Niyazi Berkes, *The Development of Secularism in Turkey* (Montreal, 1964), charts the course of the ideological ambivalence that this phenomenon creates for Turkey.

² Abdallah Laroui, *La crise des intellectuels arabes* (Paris, 1974), 21-44.

³ Charles D. Smith, "The 'Crisis of Orientation': The Shift of Egyptian Intellectuals to Islamic Subjects in the 1930's," *International Journal of Middle Eastern Studies* (hereafter *IJMES*), 4 (1973): 393-99.

upon which the elaboration of the Holy Law rested was the doctrine of consensus—the widespread agreement of the religious elite on whether a legal matter was Islamic had to be achieved before its result could be incorporated into the religious law.⁴ As the history of Muslim law records, the compulsion on the part of the ‘*ulamā*’, the men of religion, to achieve agreement led not to a constantly changing body of law but to the formation of a closed legal system at the end of the caliphal era. Thereafter men of religion ferociously defended what became a minutely defined ethical system as the cultural reference point for a civilization with a particularly high level of internal social tension.

Centuries of attempting to preserve universalism and to prevent conflict have left an intellectual heritage in the Middle East. Modern authors show a persistent tendency to mix religion with nationalism and to ignore, or to reject, the element of conflict involved in the transformation of Islamic civilization into modern societies.⁵

No less a factor in explaining the power of traditional Islamic history is the unique political role played by the culture-bearing elite. How the rapidity of the Arab conquests prevented Muslims from subjecting caliphal government to the full force of the religious movement that sprang out of Arabia in the seventh century is a favorite theme of Islamic historians. At the end of a complicated internal struggle caused by the Arab conqueror’s use of pre-Islamic ruling practices, the men of religion accepted a compromise that left them on the fringe of the political order. So long as rulers defended the Holy Law, the social and moral standards of Islamic society, the ‘*ulamā*’ legitimized a remarkably wide range of governments, making political fluidity one of the chief characteristics of Moslem government. Thus generation after generation of ‘*ulamā*’, all armed with an unchangeable religious law, came to represent what was permanently Islamic and not the offspring of ruling families.

As the Afro-Eurasian land mass became increasingly Islamic, the marginal location of the ‘*ulamā*’ within the political system made it possible for them to absorb revolutionary movements, especially those that proceeded from the less-cultured fringe of the Moslem world. All political upstarts who wished to rule on a wide basis eventually courted the ‘*ulamā*’ for the purpose of obtaining their legitimation. Successful politicians found it easy to form the alliance. An association with the ‘*ulamā*’ involved no deep commitment at the beginning to a political program short of defending the Muslim community. All that had to be accepted was the paramountcy of the men of religion in intellectual and social matters. Education was entrusted to them with few qualms because, as guardians of the Holy Law, they taught what was known rather than what might be. In addition, the ‘*ulamā*’ served both government and society as social managers, resolving conflict on the basis of their knowledge of the Holy Law. This role as peacemakers, as guardians of morality, won public respect

⁴ M. Bernand offers a short definition of “consensus,” “*Idjmāʿ*,” in *Encyclopedia of Islam* (2nd. ed.; Leiden, 1960), 3: 1023–26.

⁵ Sherif Mardin, *Continuity and Change in the Ideas of the Young Turks* (Ankara, 1969), 24–27.

for an intellectual class that tended to avoid ultimate political responsibility unless government clearly moved in an anti-Islamic direction.

When modern life shredded the fabric of Islamic society in the nineteenth century, the Community turned not to the very small number of Westernized elite for guidance but to the '*ulamā*'. Instinctively this Muslim elite responded to what was a crisis of civilization by reasserting their belief that the right path was known. Change in the modern world could not be absorbed, however, within the narrow framework that the men of religion had inherited from Islamic civilization. A small number of Muslim scholars reacted at the turn of the twentieth century to the anxiety produced by the increasing gap between reality and the vision of what ought to be by a call for reform. But the acceleration of change in the mid-twentieth century left the reform movement stillborn.

Those Western scholars who first struggled with the history of Islamic civilization did not stray far from the paths laid out by the classical Muslim historians. In one way or the other the origin of Islamic history in the West goes back to the reverent study of religious documents. To understand the meaning of obscure passages, scholars—orientalists—placed great stress upon the analysis and acquisition of Eastern languages. When orientalists reached Islamic literature, they applied these same techniques to the study of the timeless documents of Islam. Since little was known in the West about this civilization, it appeared that this cautious exploitation of linguistic abilities was indeed the correct approach. Competent scholars did not attempt to explain the sequence of events that made up the history of Islam; rather they used their prowess with exotic languages to translate Islamic civilization into Western languages.

Taken altogether the translations and commentaries of orientalists fortified the boundaries of traditional Islamic history. First, the narrow literary approach of the orientalists confined historical criticism to areas and subjects determined by a very specific kind of source. Second, their line of approach tended to identify the history of Islamic civilization with the cultural product of a narrow element within Muslim society that had an interest in suppressing diversity. Finally, given the ignorance in the West of the brilliance of Islamic civilization, translations of what the believers wrote, even when accompanied by critical introductions, added to the grandeur of Islamic civilization as it was defined by classical historians.

Two recent publications prove that the stream of translations and commentaries so necessary to the vitality of Islamic history has not dried up. Franz Rosenthal's *Classical Heritage in Islam* is an attempt, through the translation from Arabic of selected passages, to establish the link between classical antiquity and Islam. In texts that run the gamut from works on literary criticism to the occult sciences, the author shows how a creative Islamic civilization made a new synthesis in which the Greek heritage was harmonized with other cultural elements. Lenn Evan Goodman, studying the twelfth century, has translated into English a famous philosophical tale: *Ibn*

Tufayl's Hayy Ibn Yaqzān. Born on, or carried by a current to, an isolated island, the hero of this philosophical romance grew up to discover the highest intellectual and religious truths. Far from being either obscure or irrelevant, the story of Hayy Ibn Yaqzān raised that crucial issue for Islamic civilization: the relation between reason and faith. Taken together, both translations add bright new pieces to an Islamic mosaic whose general shape is already known.

Those in the West who ventured forth into the arena of Islamic history found themselves at work within a discipline whose built-in conservatism was strong. Basic language skills were so demanding that scholars who went beyond the sources were often regarded with suspicion. If orientalism inhibited experimentation, academic convention also isolated Islamic history from the more dynamic intellectual currents loose among Western historians. Housed in oriental centers, Islamic historians found their separateness confirmed by the post-World War II intrusion of government into the field of Middle Eastern studies. What the resulting intensification of support for Islamic subjects did was to strengthen the emphasis on language, on the value of inherited wisdom, and on the training of experts who would manage the relations that had grown up between the stable West and the unstable Middle East. Under such conditions the Islamic historian in the West moved closer to the intellectual attitudes of the *'ulamā'* than he did to the critical viewpoints of the modern historian.

The work of two Islamicists, H. A. R. Gibb and Gustav von Grunebaum, bears eloquent witness to the influence exerted by the men of religion even on the Islamic history written by unbelievers. Both these influential scholars adopted a universal or holistic approach to the civilization they studied. Gibb's magisterial summaries of classical Arabic literature, of Islam as a religious system, and of the attempt by the Ottomans to bring political unity to the Muslim Community are well known. His distinct lack of enthusiasm for the period when Islamic civilization became unraveled, the nineteenth century, is less recognized. It is with the work of von Grunebaum, however, that the parallelism between the way these two Western historians view the history of Islam and the manner in which the *'ulamā'* have recorded the Muslim past stands out. He believed thoroughly, as did Gibb, in the unity of Islamic civilization; he grounded his understanding of Islam in the literature of the culture-bearing elite; he avoided political and economic topics; and he disliked subjects dealing with the theme of conflict. Both von Grunebaum and Gibb felt also the need to communicate with the men of religion and even to advise them on how to cope with modern problems.⁶

This attempt to write a modern Islamic history as one grand unit was elevated to a new level of generalization with the publication of the late Marshall G. S. Hodgson's *Venture of Islam*. A major event for a scholarly field that does not produce much activity, the three volumes and 1610 pages of

⁶ See William R. Polk's contribution on Gibb and Amin Banani's contribution on von Grunebaum in "Islam and the West," *IJMES*, 6 (1975): 131-47; and Hamilton A. R. Gibb, "The Heritage of Islam in the Modern World (III)," *IJMES*, 2 (1971): 138-47.

Hodgson's analytical history of Islamic civilization from the pre-Islamic era to modern times make difficult a brief review. Fortunately, this serious American Quaker often stated his fundamental ideas.⁷ A rebel within the framework of Islamic history and an advocate on the plane of world history for the importance of his subject, Hodgson has leveled meaningful criticism against both Islamicists and other historians who have dealt in one form or another with Islamic civilization. On the plane of world history he appeals for the acceptance of a comparative viewpoint that would give Islamic civilization its rightful position of superiority between the mid-tenth and the thirteenth centuries. To prove that this ought to be, Hodgson wrote a complex and detailed history in the manner of a Persian artisan weaving an intricate tapestry of truly grand proportions.

For the Islamicist, the Hodgsonian synthesis contains a heady mixture of original ideas set within familiar parameters. Those who are concerned with his modifications of the details of Islamic history can consult the lengthy book reviews that are sure to occupy great space in many professional journals.⁸ The relationship between his core ideas, however, and the dilemmas created by the process of modernization for Islam and its history are not so complex that they cannot be treated here.

Like the men of religion, Hodgson wished to liberate Islamic history from "national" particularisms. In this case, however, it was the particularism of Western historians he criticized: the Arabists, who not only overemphasized the contribution of the Arabs to Islamic history, but also created a Mediterranean center of gravity for the entire civilization when its cultural center lay nearer the Persian plateau than it did to the Levant.

Thus, the thrust of Hodgson's work runs counter to the emphasis of Philip Hitti's *Capital Cities of Arab Islam*. Essentially a summary of the author's previous research on the history of Mecca, Medina, Damascus, Baghdad, Cairo, and Cordova, this book highlights the Arab contribution to Islamic civilization and makes no attempt to define that mysterious social institution, the Muslim city.

There would be little disagreement between Hodgson and the '*ulamā*' on the definition of the essential social group at the center of the Muslim movement. It was the community of religiously faithful, who could not be defined in local, urban, or aristocratic terms, that made Islamic history. But the wide dispersion of a non-local society, whose boundaries lay where Islam was accepted, demanded a strong integrating mechanism. Here Hodgson acknowledged the unifying role the Holy Law of Islam played in preserving the cohesion of the Community. At the same time, however, he failed to find in the inflexible legalism and the closed thinking that marked the orthodox approach to the problem of unity the factor that explained the catholic appeal

⁷ Marshall G. S. Hodgson, "The Role of Islam in World History," *IJMES*, 2 (1970): 99-123.

⁸ For a short and essentially sympathetic review, see Clifford Geertz, "Mysteries of Islam," *New York Review of Books*, 22, no. 20 (1975): 18-26.

of the religion and its civilization in its Middle Period, 1100–1500.⁹ Hodgson suggests instead, in the most stimulating portion of his work, that an increasing tension between the rigid expression of Islam (Sunnism) and the oppositional (Shiism) and the latitudinarian (Sufism) movements within the Community gave Islam its universal attractiveness.

The object, therefore, of Hodgson's hostile attitude toward the legalistic expression of Islam was not to lay the groundwork for escaping the boundaries of the classical paradigm that dominated Islamic history, but to find another explanation for its inner cohesion. Probing deeper into his argument, we find that the primal force that made this great culture the most dynamic of the cosmopolitan civilizations during the Middle Periods of its history was religious. Sufism, a gentle kind of intellectual and yet popular brand of Islam, swept through the Muslim social order after the collapse of the Abbasid caliphate in the eleventh century. Inherently pluralistic, this religious upheaval could have, given the lack of an ecclesiastical bureaucracy in Islamic civilization, dissolved the very core of Islam. Unity was preserved, however, by the appearance of a new religious mediator, al-Ghazzali, who provided a unique spiritual and intellectual compromise for a revitalized Community.

Although not a particularly new view of what happened on the religious plane in the Middle Periods, Hodgson's emphasis on the creative impulse that mysticism provided does move Islamic history away from a narrow concentration on the literary product of an extremely thin orthodox elite toward a broader conception of what constitutes Islam.¹⁰ Once Hodgson has shown the established religious doctrines to be wanting, he brings into focus the flexible and experimental face of Islamic civilization, ranging over a huge geographical arena to explore politics, commerce, architecture, and a host of other topics.

This latest, and perhaps last, synthesis of Islamic history is not phrased in the language of conflict. That Hodgson accepted the basic exterior framework of Islamic history as it had emerged in the West is certainly an internal reason for the universalistic cast of this profound effort at understanding another civilization. There are also other explanations for his sympathetic view of Muslim civilization. The piety of the Sufi surely bears witness to Hodgson's own Quaker beliefs as seen through the medium of Islamic history. His use of comparative and world historical perspectives aimed not to inflate the scale of Islamic civilization, but to humble the culture-bound arrogance of his own colleagues. Finally, his dialogue with Islamic history was yet another effort to preserve the lines of communication between those in the West who were sensitive to the *Venture of Islam* and those who seemed to be entrusted with the cultural heritage of Islamic civilization: the men of religion.¹¹

⁹ Hodgson employs a complex terminology to draw a distinction between the religion of Islam and the civilization in which Islam was the primary but not the exclusive religious phenomenon. Marshall G. S. Hodgson, *The Venture of Islam* (Chicago, 1974), 1: 3–69.

¹⁰ *Ibid.*, 2: 201–92.

¹¹ Marshall G. S. Hodgson, "Modernity and the Islamic Heritage," *Islamic Studies*, 2 (1962): 116–27.

Hodgson knew well that Islamic civilization, as opposed to Islam, came to an end sometime in the eighteenth century.¹² His untimely death, which left volume three unfinished, was tragically prophetic of the difficulties facing the discipline of Islamic history during the world-wide revolution of the modern era.

Historians of the Muslim world have recognized, in one fashion or another, that the nineteenth-century impact of the West on the Middle East disorganized both Turko-Muslim governments and societies. What was unique about this new relationship between Europe and the Middle East was the multifold internal disruption of society: Islamic institutions at all levels—social, economic, ideological, political—were attacked and destroyed or drastically modified. That this deep involvement of the West in the Middle East would eventually pull the rug from under the way Western historians perceived the Muslim past was, quite naturally, not understood or dealt with in an explicit fashion until recently. It was the study of late nineteenth-century revolutionary movements that precipitated the question; for those that were inspired by Western ideas—Turkish, Arab, and Persian nationalisms—led Islamicists to take up subjects that did not treat the Muslim world as an autonomous cultural area.

Islamic history being what it is, biographies of modern Middle Eastern intellectuals have an understandable attraction. Nikki R. Keddie's *Sayyid Jamāl ad-Dīn 'al-Afghānī': A Political Biography*, and Hamid Algar's *Mīrzā Malkum Khān: A Study in the History of Iranian Modernism*, not only are informative studies of important nineteenth-century political figures, but they also give some idea of what happens to a historical tradition caught in the throes of the modern revolution.

Half modern, half embedded in the civilization of Islam, both these men have unsatisfying, difficult histories. The first was part of the intellectual movement that introduced the vocabulary of nationalism and secularism into Iran where society, consisting mostly of aggregates of groups, had little national self-consciousness.¹³ The second flitted about the Islamic world gathering support for futile Pan-Islamic politics and for a reform of Islam that would have repudiated the consensual achievement of previous centuries. Neither a modern Islam seasoned by Western science nor Persian nationalism was at all convincing to Middle Eastern society by the turn of the twentieth century. In the short run both men were failures.

These two intellectuals escape the boundaries of Islamic history as it has been pursued in the West. Each exploited a conflict, for which there was no parallel in the past, to obtain power, and each denied both the universal and the consensual thrust of classical Islamic civilization. Did they represent the vanguard of a twentieth-century revolution? If so, what is their proper histori-

¹² Hodgson, *The Venture of Islam*, 3: 166–68.

¹³ A better example of Persian nationalism, however, would be Mīrzā Āqā Khān Kirmānī. See Mangol Bayat Philipp, "The Concepts of Religion and Government in the Thought of Mīrzā Āqā Khān Kirmānī, a Nineteenth-Century Persian Revolutionary," *IJMES*, 4 (1974): 381–400.

cal framework? Should it be secular nationalism? Malkum Khān might fit; but he was an opportunist. Should it be some form of modernized Islam? Afghānī would surely fall within such a category; but analyzing what he really meant requires the ingenuity of a master detective, something Nikki Keddie provides. The intellectual travail of both “modernizers” reveals that the old methods of Islamic history no longer apply for the nineteenth century and that the intellectual class of the Middle East is transforming itself.¹⁴

The emergence of a modern technical elite in the Middle East is now fairly well documented.¹⁵ Concerned with economic development and educational innovation—the shape of the future in a modern industrial society—these managers seek an intellectual order that is bound to have little relation to the highly structured system of religious thought that emerged in the Islamic world by the end of the Abbasid caliphate, in the thirteenth century.

It is only within recent times, however, that the intellectual interests of the modern element within Middle Eastern societies have spawned a new historiography, a young body of thought that places a great deal of weight upon the element of conflict with the outside, usually conceived of in terms of the colonial struggle with the imperialist West.¹⁶ Increasingly, however, the real historical issue in the Middle East must become an internal one. Not only has a rapid rate of economic change so differentiated the Middle Eastern states that particularism in the social and economic structures has been immeasurably strengthened, but also the need to transform Middle Eastern societies from agricultural regimes to industrial societies is sure to have an explosive effect upon the way the past has been perceived. The stress today’s economists place upon noneconomic factors in explaining the lack of development (to say nothing about the need for a new legitimacy to back tough economic decisions) points toward an intellectual war between modern and traditionalist historians in the Middle East.¹⁷

Notwithstanding Hodgson’s wish to move the history of the Islamic world from its Arab pole, events will most probably place the radical Arab historian at the forefront of ideological self-criticism. Economic and political acts have loosened the Islamic moorings of the Middle Eastern world. Yet one of the basic cultural reference points for the population of Arab, Turkish, and Persian lands is still a unitary and defensive view of an Islamic golden age. If this vision of the past is to be criticized and reinterpreted in order to encourage ideological development more appropriate to the revolutionary era in which

¹⁴ Nikki R. Keddie, “Is There a Middle East?” *IJMES*, 3 (1973): 255–71. The author of al-Afghānī’s biography did not adopt the point of view of the man she studied, but called for a history less entangled in the universalistic themes of Islamic history.

¹⁵ Unfortunately labeled the “new middle class” in Manfred Halpern, *The Politics of Social Change* (Princeton, 1963), 51–78.

¹⁶ Expressed most dramatically for Arab lands by Franz Fanon, *The Wretched of the Earth* (New York, 1963), 35–94, *passim*. For Iran, see Jalal Al-e Ahmad, *The School Principal*, trans. John K. Newton (Minneapolis, 1974), 13–18. The special appeal of Marxism in the Middle East is described by Laroui, *La crise*, 149–52.

¹⁷ *Ibid.*, 189–219, and Z. Y. Hershlag, *The Economic Structure of the Middle East* (Leiden, 1975), 1–9, 13–37, and 254–64.

the Middle East finds itself, then Arab authors are, by history, better placed to challenge the testimony of believers and to break the cake of custom. Should Western historians of the Middle East fail to perceive this change because of loyalty to an outmoded religious and literary tradition, they will surely see a breakdown of the modern dialogue.

If a metaphorical shift from consensus to conflict is to take place within the field of Middle Eastern history, it is probable that the sectarian battles of the Islamic past will take on a new interest. It is appropriate, therefore, that Julius Wellhausen's *Religio-Political Factions in Early Islam* has been translated into English. Despite the turgidity of the text—in both German and English—this old study of the oppositional movements at the dawn of Islam deals with a theme that modern historians of Islam in both the West and the Middle East should find increasingly relevant: conflict.

To invest more energy in the consensual approach to the Muslim past on the basis of wisdom received from the 'ulamā' will lead to diminishing returns. Not only has the emergence of conflict announced the defeat of the old consensus, but also Middle Eastern historians are certain to broaden the scope of their work into social and economic regions that the 'ulamā' ignored.

Publication of two important books proves that Western scholarship can expand its efforts into these two fields with great profit. Maurice Lombard's *Golden Age of Islam* is a translation from the French of his economic history of the Muslim world from the seventh to the eleventh centuries. It offers a concise treatment of a vast subject and should become a standard textbook for courses on Islamic history. *War, Technology and Society in the Middle East*, edited by V. J. Parry and M. E. Yapp, is a study of how war and military technology have changed the society of the Middle East. A timely volume, the collection of uniformly well-written papers traces Middle Eastern military history from the seventh to the twentieth centuries. Approximately a fourth of the book, moreover, shows how the modernization process placed Middle Eastern armies at the vortex of social change.

These recent publications all highlight the dilemmas faced by the Islamic historian. In the Middle East incompatible perceptions of the past reflect the ideological ambivalence of a society passing through a structural transformation of revolutionary proportions. Who the new culture-bearing elite will be and what message they will carry is unclear. Certainly few young Muslim historians will explore pre-Islamic poetry as a means of understanding their former civilization; more likely they will occupy themselves with the study of why capitalism did not appear in the Middle East. In the face of such conditions, Islamic historians in the West need not abandon their work and rush to absorb the intellectual framework of radical Middle Eastern historians. The cultural unity that Islamic civilization represented will continue to give the Middle East a cohesion Islamic historians can neglect at their peril. Yet they must also contend with the revolutionary impact of the modern differences, increasingly economic, between the Middle East and the West. It is possible for Western Islamicists to modify their narrow literary

viewpoint, to explore radical movements, to be more critical of the unity of the classical era, and, at the very least, to deal with the underdeveloped social and economic history of Islamic society from a perspective that does not rule out conflict. There is no reason why the process of modernization should destroy Islamic history in the West as a means of bridging the cultural gap between two now thoroughly interconnected societies.

Herbert Hoover: A Reinterpretation

A Review Article by ROBERT H. ZIEGER

Public Papers of the Presidents of the United States: Herbert Hoover. Containing the Public Messages, Speeches, and Statements of the President. (Published by the National Archives and Records Service.) Washington: Government Printing Office. 1974. Pp. xxxvi, 812. \$13.30.

FRANCIS WILLIAM O'BRIEN, edited and with commentary by. *The Hoover-Wilson Wartime Correspondence, September 24, 1914 to November 11, 1918.* Ames: Iowa State University Press. 1974. Pp. xxvi, 297. \$7.95.

GARY DEAN BEST. *The Politics of American Individualism: Herbert Hoover in Transition, 1918-1921.* Westport, Conn. and London: Greenwood Press. 1975. Pp. vi, 202. \$13.50.

BENJAMIN M. WEISSMAN. *Herbert Hoover and Famine Relief to Soviet Russia, 1921-23.* (Hoover Institution Publications, 134.) Stanford, Calif.: Hoover Institution Press. 1974. Pp. xv, 247. \$7.95.

J. JOSEPH HUTHMACHER and WARREN I. SUSMAN, editors. *Herbert Hoover and the Crisis of American Capitalism.* (The American Forum Series.) Cambridge, Mass.: Schenkman Publishing Company. 1973. Pp. xiii, 138. \$7.50.

MARTIN L. FAUSOLD and GEORGE T. MAZUZAN, editors. *The Hoover Presidency: A Reappraisal.* Albany: State University of New York Press. 1974. Pp. vi, 224. \$12.00.

EDGAR EUGENE ROBINSON and VAUGHN DAVIS BORNET. *Herbert Hoover: President of the United States.* (Hoover Institution Publications, 149.) Stanford, Calif.: Hoover Institution Press. 1975. Pp. xii, 398. \$12.95.

JOAN HOFF WILSON. *Herbert Hoover: Forgotten Progressive.* (Library of American Biography.) Boston, Toronto: Little, Brown and Company, 1975. Pp. viii, 308. \$3.95.

PRESENT CONCERNS IMPINGE upon analyses of the past, particularly the recent past. Events of the last decade and a half have shaken American liberals. The New Deal and social democratic traditions face heavy assault from both right and left. In their contributions to the reassessment of liberalism, historians have refurbished, not only heroes of the radical left, but conservative opponents of New Deal liberalism as well. Much of the literature on Herbert

Hoover illustrates the simultaneous resurrection of the right and repudiation of liberal policies, both domestic and foreign. Indeed, so plausible and balanced does Hoover often appear in retrospect that it requires an effort to remember his political failure and the consistency with which the American people repudiated him and his associates at the polls.

The recent literature is highly respectful of and sympathetic toward Hoover. The only clear exception is Murray Rothbard's essay in the Huthmacher-Susman book, which pillories Hoover's deviations from market economics. At the other extreme, the Robinson-Bornet volume admits of virtually no faults in Hoover, aside from his failure to pander to popular whims. Gary Dean Best and Benjamin M. Weissman seek scrupulous neutrality, but both enhance Hoover's reputation. Joan Hoff Wilson's biography combines criticism of Hoover's rigidity, self-absorption, and defensiveness with sympathy for his views and a tone of presentist special pleading for his postpresidential career. In all, the most successful efforts to attain fresh insights into Hoover's career without overcompensation are those of Ellis Hawley, whose essays grace both the Susman-Huthmacher work and the Fausold-Mazuzan collection.

Hoover's collected papers for 1929 portray the man at the apex of his career. For most of that year he was a popular president with an energetic and positive program. No longer merely an influential advisor and not yet a political has-been, Hoover shows in the papers the optimism and purpose with which he entered the presidency. Press conference transcripts, speeches, letters and statements on particular issues, and presidential proclamations record the confident public face of his administration. Extensive supplements and appendices reprint speeches from the 1928 presidential campaign and his postelection trip to Latin America. A modest spread of photographs reveals the president in his official capacities, not yet ravaged by the Depression.

The public papers contain few surprises. The prose is leaden and difficult, attempts at humor heavy and unconvincing. Energy and seriousness are equally apparent. His messages on agricultural and tariff legislation, his background press briefings, and his special letters and statements reflect thorough preparation. Hoover's statements following the conferences with industrial, financial, and labor leaders held after the break in the stock market ring with a confidence born of his detailed knowledge of the economic system. The presidential papers reveal Hoover's reliance on expert opinion, special commissions and advisory groups, and voluntary action, as well as his repugnance for legislative initiatives. The *Public Papers . . . 1929* volume is a handsome addition to the National Archives' series and begins to fill an important gap in published presidential papers.

It is difficult to understand why the other volume of documents under review was published. Although *The Hoover-Wilson Wartime Correspondence* covers over four years, most of the communications are confined to the period of Hoover's actual service in the Wilson administration—June 1917–November 1918. Many of the communications deal with mundane matters and most would be of interest only to those concerned with the day-to-day activities of

the Food Administration. Although historians have recently done much to link the two men ideologically, little in this collection touches directly on this theme. The letters and memoranda do remind the reader of the enormous complexity of food and agricultural problems during the war and reflect Hoover's decisiveness and vigor as an administrator. It is unclear, however, what broader purposes might be served by the publication of these items, especially since O'Brien's notes are largely limited to identification of references and discussion of immediate contexts. A monograph on Hoover, Wilson, and food and agricultural problems during the war would make sense; this collection of correspondence does not.

Other aspects of Hoover's early public career have already drawn monographic treatment. In *The Politics of American Individualism* Gary Dean Best follows him from his return from Europe to his entry into Harding's cabinet in 1921, while Joseph M. Weissman in *Herbert Hoover and Famine Relief to Soviet Russia, 1921-23* examines his humanitarian work as secretary of commerce. Although Best's book is unattractive in format, shockingly expensive, and narrowly focused, it makes some useful points. Carefully delineating the domestic and international strands of ideology that Hoover developed during and after the war, Best examines closely his service on the Second Industrial Conference, the abortive campaign in his behalf for the presidential nomination, and his decision to enter Harding's cabinet. Best argues that Hoover's concern with the economic consequences of the peace, both international and domestic, guided his attitudes toward the League of Nations, the labor problem, and his own political career. Seeking to promote intelligent debate over these issues and to propound his version of American exceptionalism, Hoover used his postwar fame to bring central questions of postwar political economy to public attention prior to joining the cabinet.

Although the book is solidly researched and intelligently presented, its narrow focus on three years of Hoover's life restricts its significance. Given this narrow focus and the previous appearance of important segments of the book in journal articles, this volume cannot be considered indispensable.

Herbert Hoover and Famine Relief in Soviet Russia, 1921-23 examines another aspect of Hoover's early public career. Weissman's detailed description of the American Relief Administration's enterprise focuses on Hoover's role and on the foreign-policy implications of this private body's activities. Based on archival sources, interviews, and scholarly and eyewitness literature in English and Russian, the book judiciously confronts the various ideologically tinged historical controversies associated with ARA efforts.

Weissman insists on the fundamentally humanitarian nature of the aid program. While the Hoover circle hated the Soviet regime and while some Americans were primarily concerned with trade expansion, neither counter-revolutionary purpose nor economic self-interest dominated ARA activities. Although Hoover and his associates clearly hoped that relief might encourage moderate forces in Soviet Russia, the United States government did not use

the relief mission as a device for political interference. Nor does it appear that American policy-makers sought an early detente through famine relief.

Herbert Hoover and Famine Relief is clear, direct, and cogently argued. Frequent breaks in the narrative, however, and numerous undigested quotations clog Weissman's limpid prose. The book is not, as the publishers say, a philosophic essay, but it is a solid monograph on an important subject.

If these two monographs reflect favorably on Hoover's postwar activities, his tenure as cabinet member and president has generated sharp controversy. *Herbert Hoover and the Crisis of American Capitalism* contains four essays reflecting the mixed reactions of scholars to this phase of Hoover's career. Ellis Hawley, Robert Himmelberg, Murray Rothbard, and Gerald Nash in their respective papers and rejoinders, examine Hoover's ideology and his policy choices, both as cabinet member and as president. The result is a spirited, uneven volume, unlikely to find the wide audience for which the American Forum Series is intended.

The editors provide a weak and careless introduction. Gaucheries of usage and spelling hint of haste and distractedness. The introduction contains irritating assertions that are either meaningless or questionable. Thus, we learn that among "the casualties of the First World War was the Progressive Era," a statement that is either tautological or historiographically dubious. In an apparent effort to make Hoover relevant to today's readers, the editors proclaim that his views of decentralization and voluntarism have gained new adherents, both with the right and with the New Left. This is a useful reminder, but Susman and Huthmacher confuse the issue, lumping these two categories of dissidents together and linking the resulting conglomeration with still another category of "Americans frustrated by the rise of giantism on all sides." To this hopelessly amorphous mélange of troubled people is attributed a reform program, a key ingredient of which is "corporate responsibility," a rubric that either expresses a pious hope for greater business morality or a greater regulatory role for government ("giantism"?). In any event, "corporate responsibility" may be a fond dream of good citizens, but it never sent Mark Rudd to the barricades. Against this left-right-frustrated-coalition the editors pit the "liberals of the 'unreconstructed' New Deal type [who] continue to advocate attempts to solve social problems by piling federal programs and appropriations still higher . . ."—hardly a fair statement of the goals or methods of the liberal-labor-civil rights bloc. Moreover, as Michael Harrington, Bayard Rustin, and others have demonstrated, there has been precious little real commitment of resources devoted to correcting social inequities.

Thus the editors start off with a confused and simplistic perception of the recent debate over public policy. This misleading focus is particularly annoying because this volume was commissioned expressly to stimulate debate and to introduce readers to relevant historical controversy. Left with little coherent direction by the editors, we must read the essays as separate pieces,

gleaning from them what we can. Fortunately, they are frequently informative and provocative.

Robert Himmelberg's essay is informative. It reminds readers that for all of Hoover's associationalism, he often supported antitrust action. If the dominant motif of his public career was an apolitical, nongovernmental New Nationalism, he never entirely discarded the New Freedomite fear of cartelization. During his administration key antitrust suits were initiated and won, and the Federal Trade Commission and the Department of Commerce scrutinized trade associations very closely.

Murray Rothbard's essay is provocative. A doctrinaire free-market economist, he sees Hoover as a fraud and a charlatan who compiled a record of governmental intervention in the economy throughout his tenure in the cabinet. For Rothbard, Hoover's involvement in labor matters, which other observers have seen as cautious, episodic, and limited, constitutes a coherent series of steps toward a form of corporate syndicalism. In the economist's view, America stood on the brink of fascism in 1932–33 when corporate leaders such as Gerard Swope pressed upon Hoover an NRA-like approach to recovery. In his "finest hour," Hoover rejected the Swope plan, although his career up until that point would seem to have been a preparation for it.

Rothbard sees Hoover as a practitioner of corporate liberalism, little different from any other major political figure. If even Hoover's brand of government-business cooperation and his kind of labor policies constitute drastic intervention in the market economy, the untrammelled functioning of which is vital to political liberty, the entire twentieth century—the entire modern world, for that matter—becomes a preface to fascism. If, however, there are degrees of regulation and intervention in political economy, and if governmental activity in the economic sphere is an inescapable aspect of any politics—especially democratic politics in which men and women seek advantage through organized public effort—Rothbard's essay is more dogmatic than convincing. This apocalyptic article is startling, superficially disturbing, and finally unsatisfying because it simply gives another name to actions and programs familiar under other rubrics.

The articles by Gerald Nash and Ellis Hawley are sedate when contrasted with Rothbard's sweeping claims. Nash stresses the role of science and Quakerism in the development of Hoover's ideology and contends that these forces bent him toward a moral absolutism that impeded him politically. Hawley tries to define Hoover's ideology. He stresses links to prewar progressivism and sketches Hoover's efforts to achieve a liberal capitalist order without resort to massive governmental intervention. Hawley adheres to the general view of prerevisionist liberal historians in his assessment of Hoover's performance as depression president, finding him ultimately guilty of an "intellectual rigidity [that] left him isolated from the main currents." But while Hawley's conclusions may be similar to standard liberal interpretations such as those offered by Carl Degler, Arthur M. Schlesinger, jr., and Richard Hofstadter, his immersion in the Hoover papers and his sure grasp of

twentieth-century economic policy enable him to understand Hoover on his own terms and to penetrate the complex substance of Hoover's ideology and programs. The reward of Hawley's essay lies not so much in his conclusions as in following him through the largely unexplored paths by which he arrives at them.

Hawley's depiction of Hoover as a link between power progressivism and the interest-group liberalism of the New Deal is amplified and sharpened in his essay in *The Hoover Presidency*. This volume contains nine papers delivered at a symposium at the State University College at Geneseo in April 1973. Donald McCoy's contribution stresses Hoover's political acumen in the 1928 campaign, while David Burner recalls the high hopes and systematic programs with which Hoover entered the White House. Essays by Frank Freidel and Albert B. Rollins focus on the interregnum of 1932-33. Freidel's account, a distillation of the exhaustive treatment of the subject in his biography of Roosevelt, contains few surprises, but Rollins argues that FDR had long envied and resented Hoover and had built his political career in the 1920s, in part in pointed opposition to his more successful erstwhile colleague of Wilson days. Roosevelt emerges from Rollins' account as an unusually devious and irresponsible public figure, exploiting Hoover's straitened circumstances and his forbidding public presence shamelessly in the 1932 campaign. The evidence is fragmentary at best, however, and there is no reason to abandon Freidel's more familiar version.

The heart of the volume is the section on Hoover's antidepression efforts. Hawley ably sketches both the hopes and limitations of his voluntary associationalism. The shift that occurred in the 1930s, he declares, "was not from laissez-faire to a managed economy, but rather from one attempt at management, that through informal private-public cooperation, to other more formal and coercive yet also limited attempts." Albert U. Romasco usefully surveys the Hoover historiography. He insists that a clear pattern of governmental response to economic downturn, as evidenced in 1907, 1914, and 1921-22, makes Hoover's responses to the Great Depression appear less innovative, thus complementing Hawley's effort to put Hoover's economic policies in context. Finally, in an excellent analysis based on his recent book, Jordan A. Schwartz probes Hoover's political attitudes and finds him arrogant, contemptuous of Congress, and suspicious of the processes of elective government. Schwartz traces these attitudes to Hoover's engineering background and his long and successful career as an expert-administrator whose involvement in the practical realities of business, logistics, and economic problems left him with little aptitude for, or appreciation of, messy but equally practical political realities.

The volume closes with two essays on foreign policy. In one, Selig Adler casts a skeptical eye on the recent conversion of William Appleman Williams and New Left historians to the cause of Herbert Hoover. He suggests that Williams, in his eagerness to challenge the liberal consensus that dominated foreign-policy historiography until the 1960s, may have gilded the lily in

Hoover's case, emphasizing out of context some of his anti-interventionist utterances while ignoring or slighting his economic expansionism, racialism, and strident anticommunism. Adler maintains that, however much Vietnam may have revealed the limitations of interventionism in the 1960s, the menace of fascism and militarism left FDR with little room for maneuver in the 1930s, *pace* Hoover's objections. He calls for vigorous monographic examination of the claims for Hoover's foreign and domestic policies raised by Williams.

In her essay, Joan Hoff Wilson analyzes the economic aspects of Hoover's policies, stressing the link between his efforts in this field as secretary of commerce and those he undertook as president. Wilson envisions a sure grasp of such difficult and complex issues as reparations, tariff policy, and foreign exchange. Not content with rehabilitating Hoover's reputation as a shrewd and highly informed practitioner of economic foreign policy, Wilson also suggests the perspicacity of his overall approach to foreign policy, noting that many of his strictures against overextension have been borne out. Since the essay mentions the Manchurian crisis only in passing, however, and since its substance is confined to the 1921–33 period, the last two or three pages are a gratuitous exercise in political moralism. Wilson is barely able to suggest the outlines of Hoover's response to thirty years of world events, much less to explicate the complexities of foreign policy after he left office.

The Geneseo conference must have been rewarding for its participants if the high quality of the published results is any indication. It appears that neither Edgar A. Robinson nor Vaughn Davis Bornet attended. If they did, none of the subtlety or ambiguity of recent scholarship concerning Hoover influenced their book, *Herbert Hoover: President of the United States*. This is a curious volume, a homage to an old Stanford man by two historians with long associations with that university. While it cites much of the recent literature, and while the authors interviewed hundreds of people associated with Hoover, it is a simplistic defense of Hoover's entire course of action as president. Moreover, its authors' penchant for vapid generalizations and pronouncements is befuddling and irritating. What might have been a sympathetic, tightly argued reassessment of Hoover is a pretentious apology.

The authors perceive Hoover as a man so uniquely capable and so profoundly knowledgeable as to dwarf his contemporaries. They see a public yearning for reform, for freedom from special interests. At the same time, they see the nation's political structure—its parties and caucuses, its constitutional institutions—as hopelessly provincial, self-serving, and corrupt. Little is adduced in support of these claims; they are simply asserted, often in thick, cloudy language and with gratuitous moralisms. Theodore Roosevelt, they declare, “did not accept the doctrines of the ruling elements in the Republican party. In his middle course and his pragmatic approach, he never betrayed the basic meaning of earlier American political experience. Builders of a free society must be free” (p. 7). Again: “Perhaps nothing was so characteristic of the ‘politics’ of the period 1901–1929 as the slogan ‘restore the government to the people.’ This appealed to many of the electorate who felt that

party organizations had become subservient to special interests. . . . The average citizen was quite content to believe that the managers of the great political enterprise they called the United States were the practical men known as 'politicians.' " The book is filled with such judgments that lack references and convey little meaning. A good editor might have corrected this tendency and steered the authors closer to their real purpose, informed rehabilitation of the Hoover presidency.

Such an editor might also have reduced the partisanship of the volume. A reader of *Herbert Hoover: President of the United States* would never realize that knotty, complex issues gripped the country in these years, issues over which honest people could differ and which they still debate. Everything is black and white: Hoover was invariably right, albeit at times guilty of the understandable lapses in patience that afflict a man surrounded by fools and charlatans; his critics and opponents were invariably wrong. The authors use recent research where it suits their purposes but not as a means of elevating the debate to new levels of analysis. A taste of the authors' efforts at analysis is available on the last page. They ask "why the people rejected President Hoover's program for the new scientific era, a program rooted in voluntarism and cooperation rather than federal domination. . . ." Hoover, they aver, "conducted himself as an enemy of war and armaments; as a friend of constitutional government; as a supporter of voluntary methods; as a preserver of a partially regulated capitalist system that could hold its own. . . . President Hoover . . . was, above all, a determined spokesman for the traditional American virtues, hopes, and ideals of individual opportunity, personal freedom, and love of country." The back dust jacket lists encomia for the volume by such luminaries as Thomas A. Bailey, Frank Freidel, Norman Graebner, and Ellis Hawley. But these worthies notwithstanding, the book is neither "succinct" nor "lucid." It may, as Bailey suggests, "help swing the pendulum toward a fairer and more balanced appraisal of the thirty-first President," but only for those who have read nothing on Hoover for the past ten or twelve years. The book is a serious disappointment, given the authors' access to sources and their scholarly reputations.

With all the resurgence of interest in Hoover, there is not yet a comprehensive biography. Until one appears, Joan Hoff Wilson's *Herbert Hoover: Forgotten Progressive* will serve as a brief, competent introduction to his entire career. Based as it is on newly opened archival and manuscript materials and on recent scholarship, it is more sophisticated and analytical than previous biographies. *Herbert Hoover* portrays a man who combined great practical ability and intellectual consistency with a dour and almost reclusive personality. Wilson's discussion of Hoover's ideology, noting both its relationship to other strands of progressivism and its limitations, is fair, although it frequently lacks sharpness and clarity. While her observations on Hoover's ideas and policies on domestic issues in the 1920s parallel closely those of Hawley and other recent students, Wilson's examination of Hoover's foreign policy as president and his views of war and peace in the World War II era are fresh

and particularly stimulating. An excellent bibliographical essay enhances the value of the book and partially compensates for the absence of notes, although at times materials drawn heavily upon in the text are not given attribution in the essay.

Wilson's discussion of Hoover's postpresidential career is lengthy and useful, especially in light of the lack of scholarly attention to this subject. She credits the thirty-first president with remarkable prescience for his warnings against the growth of a centralized state with vast, coercive powers and limitless international commitments. Although she acknowledges Hoover's extreme and often hyperbolic anticommunism, she notes that he never joined the McCarthyites. Her resurrection of his often trenchant and provocative critiques of foreign and military policy in the 1940s and 1950s is a great service, as is her discussion of Hoover's skepticism of bipartisanship in these areas. As Hoover did himself, however, Wilson leaves unanswered central questions about how the United States might hope to achieve the kind of expanding economy Hoover thought necessary for the maintenance of a unique social order without an aggressive foreign policy.

The chief flaw of the last part of the book lies in Wilson's largely implicit effort to enlist Hoover as a social critic. She contends that Vietnam and Watergate "have amply demonstrated his worst fears of a society and economy run from the top down by a coercive system of expertise and by what has recently been called an 'arrogant elite guard of political adolescents' " (p. 278).

This comment reflects the tone of the last part of the book. It also demonstrates some of the limitations of Wilson's prose and analysis. The use of the passive voice—a feature of her writing—obscures sources (there are no footnotes) and, in this case, substitutes a kind of cryptic rhetoric for thoughtful analysis. Hoover's penchant for expertise, his disdain for Congress, his contempt for the hurly-burly of the political process, and his compulsive self-containment hardly make him the model of the open, candid, and accessible chief executive. His faith in the public-spiritedness and ability of corporate executives certainly provided precedents for elitist decision-making. It is hard to see how either Vietnam or Watergate resulted from a "coercive system of expertise." True, Kennedy, Johnson, and Nixon relied heavily on their particular versions of the best and the brightest. But so did Hoover, whose whole public philosophy—as Wilson so correctly notes in an early chapter (pp. 72–3)—denigrated the elected representatives of the people and elevated the engineers, experts, and managers, whether governmental or private.

Moreover, Wilson seems to accept the same kind of facile assumptions shared by Susman and Huthmacher that the New Deal and postwar liberal reform have foisted upon the American people some sort of coercive welfare state without their consent and against their interests. In reality, as Hawley and Otis Graham, Jr. (in his superb essay in Elkins and McKittrick, eds., *The Hofstadter Aegis*) remind us, New Deal reforms were partial, frugally funded, closely in line with prevailing conceptions about the role of government, and extremely sensitive to the rhetoric of decentralization and local control, often

to the disadvantage of minorities, working people, and the poor. Far from creating a centralized welfare state, modern liberalism has fallen far short of its goals—always more modest than those of other Western states—in education, old-age security, health care, and many other areas.

For all of Hoover's rhetoric about grass-roots participation and the values of local decision-making, his reliance was always on elite groups. For all the limitations of post-1933 liberalism, it has been the New Deal and its heirs that have done the most to bring working people, blacks and other minorities, and the poor into decision-making processes. While it may be true that the country's international enterprises over the past generation have threatened domestic liberties, there is no easy linkage between the perniciousness of these activities and the creation of a partial welfare state. Just as the legacy of Hooverism is, as Wilson recognizes in the first two-thirds of the book, mixed, so is the inheritance of the New Deal era. If many contemporary Americans—primarily the liberals, radicals, and hirsute activists, with whom Hoover would have been, to say the least, ill at ease—might today see wisdom and good judgment in his broad strictures on foreign policy, few blacks, urban dwellers, laborites, or poor people would find much of use in his observations on government and public policy in the domestic field.

Wilson's biography accurately summarizes the interim conclusions of this round of Hoover scholarship. The limitations of Democratic liberalism, the agony of Vietnam, and second thoughts about the general efficacy of governmental solutions to social problems are conducive to favorable re-examination of men such as Hoover and Robert A. Taft. In addition, the opening of the Hoover presidential library at West Branch, Iowa, occurred when the graduate schools were swollen with eager Ph.D. candidates, many of whose lives inclined them toward a critical assessment of the previous generation's version of our recent past. At its worst, the current wave of scholarship simply updates and reinforces the self-serving and petulant tone of Hoover's own apologetics and those of his colleagues and associates in their defenses of the Chief. At its best, the current scholarship opens up to re-evaluation not merely Hoover himself, but a whole way of looking at the American social order. Let us hope that Hoover, now presumably rescued from the distortions and slurs of his political and ideological enemies, is not abstracted from his context and made to shoulder a burden of consistent public virtue and intellectual acuity that the record will not support.

Herbert Hoover rose from native stock to a position of wealth and power, first in private life, then as a public figure. Small-town Iowa and Oregon, Stanford University, the engineering profession, and private humanitarian service decisively shaped his preparation for the presidency. This background contributed personal qualities of energy, dedication, and integrity. It encouraged voluntarism, efficiency, and privatism. He never abandoned this background, nor did he transcend it. In certain respects, it would have stood the country in good stead after his fall from power; in other ways, it deserved to be rejected.

"Could it be true," asked Richard Hofstadter in his brilliant assessment of

the 74-year-old former president in 1948, “that the great American tradition was nearing its end because the people had no ear for spokesmen of the old faith?” The rise of militant unionism, agitation over civil rights, the adoption of new regulatory and welfare-state devices, and the general reliance on federal action in a wide range of activities may have resulted from devious politicians, momentary frustrations, and temporary circumstances. But were not such programs and initiatives genuinely popular and even necessary for the preservation of the social order? “Perhaps, after all,” Hofstadter wrote, “it was the spirit of the people that was not fundamentally sound.” If historians are insensitive to and impressed by the fact that Hoover and his social vision have been frequently and resoundingly repudiated by the American electorate, they may find themselves sharing with Hoover, not only the more attractive aspects of his public thought, but some of the implicit distrust of the people that lurked beneath the surface of his rhetoric.

Reviews of Books

GENERAL

KENNETH WINETROUT. *Arnold Toynbee: The Ecumenical Vision*. (Twayne's World Leaders Series.) Boston: Twayne Publishers. 1975. Pp. 155. \$7.95.

Arnold Toynbee's arena has been the world, or perhaps more grandly, the cosmos. As Kenneth Winetroun notes in this study, "For Toynbee, history becomes a vision of God revealing Himself in action. . . . 'Man's *Oikumene*' is a 'fragment of God's Universe'."

The aim of Winetroun's brief but useful introduction is to demonstrate Toynbee's commitment to this view. Winetroun's analysis principally concerns *The Study of History*, that multivolume, alpha and omega history of mankind that contains the particulars of Toynbee's ecumenical vision of history: "God revealing Himself in action." From this vision comes Toynbee's conclusion in *The Study of History* that religion rather than civilization is the intelligible unit of historical study. *The Study of History* is the history of God more than of the world He created; the thinking behind it is at once historical and philosophical. As understood by Winetroun, *The Study of History* presumes a view of history as pattern, imagination, curiosity, feeling, contemporaneity, myth, theology, and prophecy; history further is comprehensible only in the long view and universally.

Winetroun is coherent in style and explication, and he argues well for Toynbee's place in the world of scholarship, thought, and criticism. One chapter compares Toynbee and Spengler on the future of the West; another treats Toynbee on the future of man. Here is a key area, as Toynbee says: "My study of the world will have been barren and irresponsible if it has not equipped and spurred me to do what I can—infinitesimal though the effect of my action may be—to help mankind cure itself of some of the evil that, in my lifetime, I have seen human beings inflict on each other." Winetroun further discusses how Toynbee extended this presentism to analyze the dynamics of change, and he concludes with an evaluation of that all-important

point, Toynbee's personal relationship with religion.

The book is limited to Toynbee's primary theses and the philosophical-cum-historical foundations upon which they rest. The treatment of philosophy seems sound, as does Winetroun's discussion of Toynbee's historiography. If there is a major flaw it is that Winetroun is too reverent, treating Toynbee with awe while almost curtly dismissing his critics. Nevertheless, the awe is understandable: it is that of one confronted by a giant intellect and a great man of historical thought.

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LEON J. GOLDSTEIN. *Historical Knowing*. Austin: University of Texas Press. 1976. Pp. xxvii, 242. \$10.00.

BERNARD LEWIS. *History: Remembered, Recovered, Invented*. Princeton: Princeton University Press. 1975. Pp. 111. \$6.95.

Until World War II philosophers in this country and Britain seldom wrote on the philosophy of history. Since the war, however, many have, and two dominant schools have emerged. The first has argued that historians make sense of history by drawing on general laws. The second has portrayed history as a kind of narrative discourse whose chief feature is its credibility. Neither school has struck most historians as particularly fruitful.

In *Historical Knowing* an American philosopher, Leon J. Goldstein, shows why neither can be. He deals at greatest length with the general-law theorists. They have held that historians appropriate general laws from natural science or mundane experience to explain the past and have insisted that the past which historians seek to explain has an objective existence of its own. Goldstein denies both assertions. He demonstrates that historians think inferentially, not deductively, and he brilliantly demolishes the notion that there is a real past. He notes, for good measure, that the examples that general-law theorists give us of historical explanation are remarkably vague and un-

persuasive. In his attack on the exponents of the narrationist school, Goldstein is briefer but equally effective. He shows that their definition of history—that it is something marked by credible narrative—leaves history indistinguishable from fiction and naively ignores the way in which historians use evidence.

In all this Goldstein's chief goal is to demonstrate that historians think in a way which recent philosophers have completely failed to understand. Goldstein uses a number of examples to make his point, and some of these may leave the reader squeamish. A. J. P. Taylor is presented as having selected evidence in a way no more arbitrary than any other historian in his *Origins of the Second World War*, and Arthur Schlesinger, Sr. and F. Lee Benson emerge in virtual agreement over the nature of Jacksonian democracy. Goldstein might profitably pay more attention to the nature of historical controversy than he does. Yet this is a small criticism of a book that does a splendid job of clearing away two profitless theories of historical knowledge.

If Goldstein sought to determine what historical thinking is, Bernard Lewis shows us how irresponsible historical thinking can be. Citing Near Eastern examples, he demonstrates how current needs have led men to warp the past virtually beyond recognition. Lewis argues that this began when Near Eastern intellectuals, in their search for a glorious past that would compensate for current weakness, selectively expropriated the work of Western scholars and novelists. Then, with the collapse of the Ottoman Empire and Western colonialism, these intellectuals developed their views of the past to the point where, in highly colored form, they often serve reckless or reprehensible political policies.

In describing the rise of ethnocentric and nationalist historiography, Lewis tackles a question that Goldstein does not consider: the way in which present circumstances can shape not only the questions that men ask of the past, but also the answers they give. Like Goldstein, Lewis believes that the historian should go where his evidence takes him. Unlike Goldstein, Lewis knows that the historian (or lesser mortal) often does not. In framing his picture of how the past has been conveniently resurrected and touched up in the Near East, Lewis strongly suggests that the same has taken place, or is taking place, elsewhere. There seems little question that this is so; the warning that the abuse of the past can be dangerous, although scarcely news to historians, is presented in a fresh and persuasive way.

STEPHEN BAILEY
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LÉON-E. HALKIN. *Initiation à la critique historique*. (Cahiers des *Annales*, 6.) 4th rev. ed.; Paris: Librairie Armand Colin. 1973. Pp. 267. 25 fr.

It is only fitting that such a solid and prolific contributor to European historical scholarship prepare, more than forty years after his splendid study of Cardinal de La Marck appeared, the present study of history. He defines history as "la discipline qui étudie le passé des hommes et présente un tableau raisonné de leurs actions de portée sociale."

As Léon-E. Halkin points out in the *avant-propos*, this work is not intended as either a synthesis or a manual but rather as a presentation of "a selection of appropriate ideas and examples in order to understand better the difficulties [involved in the study] of history and of the historian's mission." He also points out something elementary that is too often forgotten: "The important thing is not so much to learn to write history, as to learn to read it intelligently." With these ideas in mind the author prepared a book divided into two sections: the first he calls "Notions de Critique Historique," and the second, "Applications de la Critique Historique." The first part deals with chapters on the history of history, historical reality and historical truth, methods and rules of historical criticism, hypothesis and synthesis, historical documents, the auxiliary sciences of history, history and philology, biography, and so forth. In the second part Halkin reviews the applications of history to such subjects as Erasmus, Philip II, the myth of Napoleon, and Hitler.

In this work, intended essentially for students, the author presents a clear and well-organized approach to the subject of historiography. He refers to traditional European historians and their contributions, but he ignores more or less completely all non-European historiography. As would be expected of a first-class medievalist, he stresses time and again the necessity of using primary sources, which today is, of course, taken for granted as an axiom, but not always practiced. Halkin continues that it is not always easy to analyze history; in fact for some "it is easier to scorn history than to pass in judgment over it." He contradicts himself only once when he states that "history is to the humanities what mathematics is to the experimental sciences: a guarantee of precision" (p. 49), whereas a few pages later (p. 51) he states that "there is no *one* history" and on the same page adds that "the duty of historians is precisely to make critiques of personal approximations and then to reduce them as much as possible."

On the causative interpretation of history, the author uses the example of the outbreak of World War II and the rise of Hitler to stress that there is no "one cause" of the great events in history but rather "hundreds of factors, known and unknown." Analyzing history in general and its value to us, he says that it "no longer claims to reign over the future or the present, nor even over the

past. However amoralistic this may be, it [history] does maintain an educational value—for those at least who can understand it—in the simple fact that it helps us to understand men and life. It is important that it [history] does not stop corresponding to that tenacious aspiration called ‘the memory of mankind.’ It is not the function of history to crush man beneath the weight of its past, but rather, to show this past and to remind man, discretely, that we are its heirs.”

Halkin’s study is a valuable contribution and the summation of a life dedicated to the study of history. If this study says little that is new or unexpected, it will nonetheless be welcomed by peers as well as by the newer generation of scholars.

ALAN SCHAM
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WILLIAM A. GALSTON. *Kant and the Problem of History*. Chicago: University of Chicago Press, 1975. Pp. xiv, 290. \$13.50.

This work is a critical examination of Kant’s philosophy of history and its influence on the doctrine of historicism, which William A. Galston defines as “the doctrine that every human situation is circumscribed by a historical fatality that is invisible, unknowable, and arbitrary in the sense that it is not the inevitable or predictable outcome of any preceding situation” (p. 4), and on contemporary political doctrines, such as nihilism and revolutionary idealism, that presuppose this definition of historicism. Galston attempts to show that Kant’s views on history, too neglected until recently, not only anticipated the theory and practice of modern liberal democracy, but also helped to precipitate what he calls “the contemporary crisis of liberal democracy” (p. 1). Galston proceeds by analyzing in detail Kant’s two most important writings on history—“Conjectural Beginning of Human History” and “Idea for a Universal History from a Cosmopolitan Point of View”—and comparing them with the views of Aristotle, Aquinas, Rousseau, Hegel, and others. Galston concludes that Kant’s irresolvable dualism rendered his concept of history “incapable of performing the task for which it was designed: the theoretical mediation between a natural world indifferent to human will and human will impotent to transform that world in accordance with its moral intention” (p. 269).

The author’s critique of the essays themselves and of Kant’s brilliant but ultimately unsuccessful attempt to reconcile his views on nature, morality, and history is especially good. Less satisfactory are his lengthy, largely irrelevant discussions of Aristotle and Rousseau, which add little to our understanding of Kant’s uniqueness or significance. And, for purposes of comparison, why no discussion of Leibniz, Hume, or Herder, who are far

more important for an understanding of Kant than Aristotle or Aquinas? Moreover, the writing is uniformly tedious and sometimes unclear. On balance, however, Galston’s first book is a worthwhile contribution to a neglected but significant side of Kant’s philosophy.

ROBERT ANCHOR
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RUSSELL MCCORMMACH, editor. *Historical Studies in the Physical Sciences*. Volume 5. Princeton: Princeton University Press 1975. Pp. 185. \$12.50.

The authors of this unique and often fascinating volume on “everything you wanted to know about the cost of academic physics at the turn of the century but were afraid to ask” have provided a very useful collection of information. They have discovered, and at times created, figures for a multinational account of the numbers and growth rates of the population of academic physicists, students, and support staff; the personal income of physicists; laboratory budgets and special research funding, including the costs of major research tools and materials; the cost of new physical plants; and the productivity of physics papers per physicist and per unit of investment. To facilitate comparisons, all monetary figures are given in 1900 German marks—roughly equivalent in purchasing power to American dollars in 1960. The ingenuity displayed in establishing educated guesses for many of the figures is both wonderful and worrisome, and it demands that the reader use substantial caution in using the data presented.

Much of the information gathered here confirms the conventional wisdom, but there are some important surprises. By 1900 the United States led all major nations in total number of physicists and physicists per capita, and it was a close second to Great Britain in total expenditure per physicist. This result is particularly startling because the authors’ method of counting physicists probably excludes many astrophysicists who held posts in astronomy. Because this field was both cost intensive and especially large in the United States, American numbers and support levels are probably understated with respect to those of other nations.

In spite of my reservations about the reliability of some figures, if parallel studies approaching this in quality are done for other sciences and for other times, they will greatly enrich our understanding of the institutional settings of the scientific enterprise.

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OWEN HANNAWAY. *The Chemists and the Word: The Didactic Origins of Chemistry*. Baltimore: Johns Hopkins University Press, 1975. Pp. xiii, 165. \$10.00.

Opportunities for formulating and answering basic historical questions are found in the origins of textbooks. The beginning and rise of chemistry as a unique science in the seventeenth century grew out of Andreas Libavius' (1560–1616) commitment to the didactic tradition that "man's arts be reduced to systematic presentation in words" (p. 116). Libavius brought logic, organization, and a concern for correct terminology to bear on the processes of chemistry that included extracting pure juices, mixing and separating substances, and changing and transmuting things. To do this he felt compelled to distinguish chemistry from its applications, such as mining, tanning, and medicine. In facing up to the difficulties in selecting and organizing the variety of processes common to the art of chemistry, he turned to the widely understood art of dialectic, which is "of discovering simple notions and of unveiling them in order for judgment" (p. 124).

The role and contribution of Paracelsus (1493?–1541) to the origin of chemistry is reduced to a foil against which Libavius argued by challenging the Paracelsian philosopher Oswald Croll (1560–1609). Croll's book, *Basilica Chymica* (1609), provides Paracelsian ideas most clearly and is the best example of the Paracelsian chemical pharmacopoeia. The underlying difference between Croll and Libavius was the role of language in the explanation of nature that each espoused. Paracelsus and Croll emphasized the craft of chemistry with all its laboratory complexities, while Libavius stressed the written formulation of chemistry as a discipline that could be taught. From the experience and experiment urged by Paracelsus and the methodization insisted upon by Libavius the first textbook of chemistry was written, Libavius' *Al-chemia* published in Frankfurt in 1597.

Owen Hannaway has clearly and cleverly presented the spectrum of intellectual forces (Ramusism, Rosicrucianism, Calvinism, Lutheranism, and so forth) that shaped the earliest chemistry textbook. His text is well presented, succinct, and physically pleasing. Its merits make it a book to be read by historians. It will undoubtedly stimulate a new chapter of discussion and research, if not tradition, in the history of science that will make this specialty topic a notable chapter in the history textbook for this age.

AUDREY B. DAVIS
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STEPHEN KERN. *Anatomy and Destiny: A Cultural History of the Human Body*. Indianapolis: Bobbs-Merrill Company. 1975. Pp. xii, 307. \$10.95.

This is a historical survey of some nineteenth- and early twentieth-century attitudes toward the body

and its functions. Stephen Kern was a research fellow in psychiatry while completing his dissertation in history. The impetus for the book came from his belief that Sigmund Freud's work was only part of a much broader cultural assault on the repressive sexual morality of the nineteenth century. The author attempts to combine psychological insights with the methods of the historian. The book reads well and is amply documented. It also includes considerable information not readily available elsewhere. All of this is to the good. My overall impression of the book, however, is a negative one.

My essential criticism is Kern's lack of historical background for the subjects he discusses. He assumes that many of the things he mentions are unique to the nineteenth century, and this has led him into serious errors. An example of this is Eduard von Hartmann's belief (1895) that if a mother conceived a child with a second man, the child would show the influence of the first husband. Contrary to Kern, this idea was not unique to the nineteenth century, but also circulated widely in the seventeenth century. Kern discusses the new attention paid to the olfactory impressions without realizing the whole public health and sanitation background summarized by George Rosen and others. Kern states, without realizing the conflicting and contradictory trends, that the sexual morality of the early nineteenth century was essentially that of the bourgeois. He does not understand the implications for Victorian sexual morality of the discovery of the third stage of syphilis; he confuses *coitus reservatus* with *coitus interruptus*; he ignores much of the history of bathing; and he misses the social implications of tight lacing. This list could go on.

In defense of Kern, it might well be that the difficulty does not entirely lie with him, but that his research is in a comparatively new field in which there are few guideposts and a general lack of basic monographs as well as overall views for the historian to use as points of reference. Thus each of us tends to create *de novo* rather than building upon the work of our predecessors. Kern is to be commended for his attempt. I am sad to report that he has not been notably successful.

VERN L. BULLOUGH
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Northridge

STEPHEN GARDINER. *Evolution of the House: An Introduction*. New York: Macmillan Publishing Company. 1974. Pp. xiii, 298. \$10.95.

This series of ten essays, with a conclusion, was written by Stephen Gardiner, a British architect, to show laymen readers the practical ideas behind

house development through the ages. Decidedly a present-day architect's perspective, its principal concern is with the enclosure of space. The book's primary weakness is its failure to show to any extent how ways of living influenced house layout and design, and there is virtually no mention of interior decoration, which can be more revealing than the architecture itself, especially in our own time. It would have been a better book for having done those things; but its merits as it stands largely repair the damage of omission. The author's topics are direct and to the point: "Outlines," "Solids," "The Frame," and "The Garden." All of the essays support his thesis that architecture today has ceased to be humanistic, as it was in the vernacular, and his conviction that a return is possible within the context of modern life. Except for the latter part, the thesis could have burst whole from the English Queen Anne movement of the 1870s. In addition to using photographs, Gardiner has helpfully illustrated his book, in the style of the English, with simple pencil sketches.

Evolution of the House is the best and clearest single volume on the subject available today. Readable and often very witty, it can be appreciated by students and the growing number of house enthusiasts as a guide to the shelves of more detailed monographs, which are more often than not hamstrung by their own jargon.

WILLIAM SEALE
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JOHN HIX. *The Glass House*. Cambridge, Mass.: MIT Press. 1974. Pp. 208. \$22.50.

Because glass is transparent to light but opaque to heat, glass structures can serve as a solar energy trap. This greenhouse effect, combined with inexpensiveness and beauty, has prompted widespread interest in glass as a building material among architects who, like John Hix, believe the central purpose of architecture is creating artificial environments. Efforts to exploit the unique properties of glass go back a long way, however, as Hix discovered in 1969 when he and his students at Cambridge University built an experimental glass home. Background material gathered by students as part of the exercise aroused Hix's curiosity about antecedents and led him to compile the pictures and chronological notes presented as a history of the glass house.

The book has two parts. The first concerns European attempts to provide artificial climates for exotic plants, mainly during the eighteenth and nineteenth centuries, and it concludes with a survey of the current status of horticulture under

glass. In the second part of the book, Hix turns to the glass house as habitable space. He traces the nineteenth-century development of prefabricated glass and iron structures, stressing the construction and significance of the Crystal Palace and its successors. The closing chapters deal with recent experiments in glass homes and with larger plans for glass and plastic-enclosed towns.

The character of the book is mainly defined by its illustrations, 317 of them accounting for well over half its contents. Each print, plan, and photograph is clearly reproduced, informatively captioned, and carefully documented. The text, rather less fully documented, is perhaps best understood as supplementary to the illustrations. This is no comprehensive history of glass houses, nor was this the author's intention. What Hix offers is a visual essay with commentary on some of the ways glass has been adapted to changing needs and tastes in building over the past two centuries. The result is an attractive and suggestive introduction to an unfamiliar aspect of architectural history.

BARTON C. HACKER
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PIERRE VILAR. *A History of Gold and Money, 1450-1920*. Translated by JUDITH WHITE. Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 360. \$18.50.

JOHN KENNETH GALBRAITH. *Money: Whence It Came, Where It Went*. Boston: Houghton Mifflin Company. 1975. Pp. 324. \$10.00.

Pierre Vilar's *History* is primarily a work of summary and synthesis, written for a Spanish and French audience. Newly translated into English, it partially fills a gap in our own literature by surveying the evolution of metallic monies over the span of five centuries commencing with Columbus—"partially," because it is too brief a book to wield effectively so broad a brush. But the bulk of the work develops a reinterpretation of the sixteenth century, when a flood of gold and silver streamed from the mines of Peru and the West Indies to the markets of Castile and thence throughout Europe and later into Asia. The standard work in English on that critical episode remains that of Earl Hamilton (*American Treasure and the Price Revolution in Spain, 1501-1650*), who treated the discovery and shipment of the metals as an autonomous force that dramatically altered prices and then the whole scope of economic activity in Spain.

Vilar's contribution is to emphasize the relationship of these discoveries—and monetary change in general—to antecedent conditions. In the second half of the fifteenth century the conflux of demographic expansion, technological change in agriculture and industry, the development of the large

commercial fair, and the spread of nationalism had generated an economic revolution that preceded and stimulated the quest for gold. By 1492 goods were available cheaply in Europe to anyone with gold to buy them. The voyages of Columbus, Vilar argues, were largely motivated by this economic conjecture. He concludes that in "economics as in history, there is no such thing as unilateral causation . . . : economic activity attracted gold and silver, and they in turn were an incentive to further activity" (pp. 255, 262). This conclusion serves not only as an aid to understanding the sixteenth century, but also as a lesson to those economists who claim a central role today for money as a cause of economic change.

One always hopes for the best from a new book by John Kenneth Galbraith. He is the *premier* writer among living economists, the master of the pungent epigram and the amusing anecdote. In *Money* he focuses these formidable talents on the demystification of monetary history, wrenching it from the fists of the *cognoscenti* and presenting it to the general reader. He attempts, as does Vilar, to summarize the post-Columbian development of money. He emphasizes America more than Europe, banking more than metals, and the past two centuries mainly, ending in 1974.

Galbraith, a past president of the American Economic Association, obviously holds impeccable professional credentials and should not be readily dismissed. And yet his writing here sometimes merely simplifies complex issues beyond recognition. He anticipates the criticism at the outset, informing us that "whatever errors . . . this history may contain, there are . . . none that proceed from simplification" (p. 5). But then he writes *inter alia* that "money is nothing more or less than what [the reader] always thought it was" (p. 5), that "the Federal Reserve System . . . enjoys the liturgy but not the reality of independence" (p. 30), and that the 1973-74 rise in oil prices did not contribute to inflation: "In fact, it was deflationary" (p. 299). A treasure of subtlety has been crushed beneath the weight of these assertions.

Galbraith titillates with savage and often unsupported attacks on the famous: Ludwig Erhard was "an accidental man" (p. 162); Richard Nixon "invites and justifies all possible criticism" (p. 285). He concludes his discussion of American policy with a blanket condemnation of those who have managed our money ("the task attracts a very low level of talent" [p. 302]), though his survey has not once mentioned either of the men who have dominated United States monetary policy for the past twenty-five years: William McChesney Martin and Arthur F. Burns. Elsewhere, *Money* is often fair, and it is entertaining throughout. But ulti-

mately the book is a placebo and should appeal only to those who feel they cannot digest a more serious discussion of monetary history.

JAMES BOUGHTON
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RAPHAEL SAMUEL, editor. *Village Life and Labour*. (History Workshop Series.) Boston: Routledge and Kegan Paul. 1975. Pp. xxi, 278. Cloth \$21.75, paper \$10.95.

This product of an experiment in alternatives to the examination system for older, working students offers samples of "people's history" based on a variety of primary source materials. On the basis of this first volume in the series, the experiment must certainly be judged successful.

The first three parts, a general essay by the editor, a study of village harvesters by David H. Morgan, and a study of country work girls by Jennie Kitteringham, are excellent. They explore the diversity of nineteenth-century rural life, the fluidity of occupational boundaries, the economic role of the family unit, the importance of cottage industries, and the seasonal migration of workers in search of jobs. The social upheavals and physical demands imposed by the urgency of harvest periods are not neglected, as is often the case, and it is also refreshing to find women and children quietly taking their place in social history without cant or cavil.

Although it explores similar themes, the last and longest part of the book, a study of a quarry village and an essay on oral history by the editor, fits less smoothly into the overall structure. It focuses on a slightly later period than the preceding essays, extending into the first two decades of the twentieth century. Given the nature of the source materials, this section might have explored some important areas in more detail than was possible in other essays. His examination of income, diet, and family relationships lacks the rigor and vitality of the earlier essays, and the fact that in this village the cottage laundry industry was admittedly as important as the brick works makes his comparative neglect of the economic roles of women and children annoying. Unfortunately, although interesting and often illuminating, Samuel's essay tends to be anecdotal rather than analytical.

Despite the occasional disappointments of the final section, this is a lively, valuable addition to nineteenth-century social studies and an essential work for those interested in rural life and work.

JANET ROEBUCK
University of New Mexico

E. J. HOBBSAWM. *The Age of Capitalism, 1848-1875*. (History of Civilization.) New York: Charles Scribner's Sons. 1976. Pp. xv, 354. \$17.50.

E. J. Hobsbawm's *Age of Capitalism, 1848-1875* is part of an English series entitled the History of Civilization. This fact is not prominently displayed by the publishers probably for fear that, as part of a series, the book will be classed as a textbook and given but slight consideration. Such things have happened, which is a great pity, since at present some of the best historical writing, by the most competent authorities, appears in this form. This is the day of microscopic research, when most of the synthesis is provided by textbooks. Books such as the present one, reminiscent of Burckhardt's attempts at cross-section analysis and synthesis, are of great importance not only for the student but for the educated layman.

Hobsbawm's book is a superior piece of work, comparable to Robert C. Binkley's *Realism and Nationalism, 1852-1871*, published forty years ago in the Rise of Modern Europe series. As editor of this last-named series, I will be excused if I look at the two books with an eye to the changes that have taken place in historical writing over the past half century.

The most striking difference is that while Binkley dealt only with European developments, Hobsbawm takes the entire world as his province. This is as it should be in light of the rise and fall of imperialism. A second noted difference is that while Binkley dealt first with science, religion, and the arts before turning to economic developments and social problems, Hobsbawm has reversed the order. Here again his approach is probably sounder, for, as he points out, after the flare-ups of 1848-49 "the Industrial Revolution swallowed up the French Revolution." The decade following 1850 saw an unparalleled development of industry, transportation, and commerce. Hobsbawm is a confirmed Marxist, so it is easy for him to elaborate on economic developments and their consequences. I hasten to add, however, that he is not painfully Marxist.

While I admire Hobsbawm's book for the wealth of information and the logic of argument, I cannot help but feel, as a man of an earlier generation, that to really understand a period, a certain amount of systematic, chronological knowledge is desirable, if not essential. Hobsbawm tends to skip to and fro as his argument requires. Traditional events of the period are barely sketched, and the problems and policies of governments and statesmen are mostly omitted. His whole tendency is to personalize the forces of history and to categorize society even to the point where at times it seems

highly artificial. The "working classes" (whoever they may be) appear as the "helpless victims" of ruthless, brutal forces, unregulated and unchecked by governments.

Details on which I might differ from the author seem too insignificant to warrant discussion or even mention. Hobsbawm is far more competent in the field of general history than many economic historians. He never overplays his points and never loses from sight "the global triumph of capitalism" that is indeed "the major theme of history in the decades after 1848." His book is a splendid achievement, certainly the most up-to-date and comprehensive treatment of world history in a period of critical transition.

WILLIAM L. LANGER
Harvard University

THADDEUS J. TRENN, edited with commentary by. *Radioactivity and Atomic Theory*. Presenting facsimile reproduction of the *Annual Progress Reports on Radioactivity, 1904-1920, to the Chemical Society* by FREDERICK SODDY F.R.S. New York: Halsted Press. 1975. Pp. xv, 517. \$35.00.

The Chemical Society of London inaugurated the tradition of the *Annual Reports on the Progress of Chemistry* in 1904, publishing yearly volumes under that title, each of which contains reports of current problems and researches in various fields of chemistry. Radioactivity was then a new and exciting area of research, and among those in England most active in its pursuit was young Frederick Soddy who had worked with Ernest Rutherford in developing the atomic disintegration theory of radioactivity. In 1903-04 Soddy was confirming spectroscopically the production of helium from radium with William Ramsay, a noted chemist and a member of the publication committee for the *Annual Reports*. Soddy agreed to write the first report on radioactivity and continued to contribute these almost yearly until 1920. He received the Nobel Prize in chemistry in 1921 for his work, which included investigations into the origin and nature of isotopes.

The core of *Radioactivity and Atomic Theory* is a facsimile reproduction of the radioactivity reports. Complementing these are Thaddeus Trenn's tables of errata, appendixes, and bibliography, as well as subject and author indexes, the latter compiled and occasionally amended from indexes to the original volumes. This core is preceded by a forty-four-page introductory commentary with sections that direct the reader to those pages of the reports where various topics appear. The introduction indicates problems and theoretical de-

velopments linked with the designated topics. Its references are keyed to the original pagination and volume numbers of the *Annual Reports*, and these appear in addition to the regular pagination used throughout Trenn's entire text.

The major deficiency of the book is its failure to provide a modern historical commentary on radio-activity researches during the years of Soddy's reports. An eight-page "historical overview" in the introductory commentary is a reproduction in facing German and English of a 1923 resumé by F. A. Paneth and György Hevesy. A "biographical account" of Soddy is a short extract from *Les Prix Nobel en 1921-1922* (Stockholm, 1923). Although there are allusions in the introduction to the "interaction of experiment and theory," "theory-building," "experimental anomalies," and so forth, Trenn has clearly intended this volume as a source book and no more. The failure to provide any historical interpretation is in my opinion a shortcoming of the volume, and it is particularly disappointing since Trenn is eminently qualified to write a historical introduction.

MARY JO NYE
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STEPHEN R. WARD, editor. *The War Generation: Veterans of the First World War*. (National University Publications: Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 192. \$12.50.

The purpose of this slim volume of five essays is to analyze the activities of World War I veterans in five nations: Great Britain, France, Germany, Italy, and the United States. A particular concern has been to discover whether, as some suggest, military experience creates nostalgia for the "military way," leading veterans to favor rightist ideologies. These essays argue against such a broad generalization. In his introduction Ward points out that in each of the four European countries studied, some veterans supported leftist causes. As for American veterans, Ward might have given attention to an earlier study (Rodney G. Minott, *Peerless Patriots: Organized Veterans and the Spirit of Americanism* [1962]), which maintains that enthusiasm for jingoistic nationalism could well have resulted from the control over organizational policy by "professional veterans" at national headquarters, rather than from Fascist tendencies derived from authoritarian army experience.

The editor finds similarities and differences in each of the countries. One obvious similarity is the importance of persuasive leaders, all of whom "used similar rhetoric, albeit expounding different political views." Where Mosley, Barbusse, and

D'Annunzio failed, Mussolini and Hitler enjoyed spectacular success. (Michael A. Ledeen's chapter leads one to wonder, however, what might have taken place in Italy had D'Annunzio shown more decisiveness in leading the non-Fascist *arditi* veterans, and had he not fallen or been pushed from that window at a crucial moment in the developing national crisis.) Another similarity was the tendency of veterans to take part in disruptive incidents "either initiated on their own behalf or joined by them in support of other causes." James M. Diehl shows how rightist veteran organizations and combat leagues contributed importantly to the collapse of the Weimar Republic; Robert Soucy points out that many veterans were involved in the Paris riots leading to Daladier's resignation; Donald J. Lisio emphasizes the serious political damage that Hoover suffered for his mishandling of the most important instance of American veteran activism, particularly for his blind acceptance of MacArthur's subsequently disproved "communist-threat" excuse for driving out the bonus marchers in defiance of the president's instructions. The tendency among a number of associations to divide according to ideological preferences was another similarity in the three Continental nations. In sum, Ward finds the differences to be more subtle than the similarities. Moreover, he emphasizes the much greater radicalism in Italy and Germany than in Great Britain and the United States.

The five authors, perhaps, should have expanded their essays. Within the limitations of space, however, the results are satisfactory; all five essays are well documented and eminently readable. And one must agree that in its purpose the collection is, as the dust jacket boasts, unique.

MARY R. DEARING
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MARTIN JAY. *The Dialectical Imagination: A History of the Frankfurt School and the Institute of Social Research, 1923-1950*. Boston: Little, Brown. 1973. Pp. xxi, 382. Cloth \$12.50, paper \$3.95.

For Marxists, and perhaps everybody, the central fact of twentieth-century life is the range of strategies by which capitalism has evaded the sentence of doom pronounced upon it by Karl Marx. The administered welfare state and fascism are two of the most ingenious. Many observers would add Soviet socialism. In the process, the revolutionary proletariat has been assimilated by bribes and behavioral engineering into a new mass culture that at best only parodies Marx's hopes for a liberated humanity, and at worst surpasses the hellscapes of Yevgeny Zamyatin and George Orwell.

In *The Dialectical Imagination*, Martin Jay supplies a lucid history of a movement in modern radical social philosophy that has confronted Marx's prognostic shortcomings with an uncommon mixture of sympathy, irony, and openness to both pre- and post-Marxist thought. The Frankfurt School, and the *Institut für Sozialforschung* out of which it grew, developed in its prime a multidisciplinary critique of modern civilization that ranged from the loftiest peaks of speculative philosophy through esthetic theory and *Geistesgeschichte* to inspired works of empirical sociology. Its inner circle, the true dialecticians, consisted preeminently of Max Horkheimer, Theodor Adorno, and Herbert Marcuse. The middle and outer circles included such remarkable scholars as Walter Benjamin, Erich Fromm, Franz Neumann, and Karl August Wittfogel.

Jay's book, based on his Harvard dissertation for H. Stuart Hughes, is one of the few obviously major works in intellectual history by a younger scholar to appear in this decade. His eight well-wrought chapters follow the history of the Frankfurt movement from the early Weimar years to the triumphant return to Germany in 1950. Along the way are detailed analyses of its critical theory, psychosocial research, dissection of mass culture, and philosophy of history.

The greatest single curse of Frankfurt social philosophy was no doubt the denseness of its prose. This becomes a manageable problem in *The Dialectical Imagination*. With little sacrifice of the rich and deliberate ambiguities issuing from the school's dialectical method, Jay translates most of its Hegelian-Marxian-Freudian jargon into good academic English, with a thoroughness that should leave only the most inattentive reader still at sea. The author earns our thanks. It remains unhappily true that in its struggle not to be crude, vulgar, and simplistic, the Frankfurt School too often ended by being obscure, elitist, and labyrinthine.

The difficulty of the school's language, of course, stemmed in part from the difficulty of its concepts. If many scholars, especially outside the German-speaking world, were long unable or unwilling to negotiate the heights of abstraction scaled by Horkheimer and his associates, this was a price that most of them felt obliged to pay. They saw their mission as essentially critical: to break through the crystalline masses of Marxist dogma encrusting the modern radical mind, and with the help of dialectical reason to promote freewheeling modes of thought that would explain the tragic deviations of social democracy from the course plotted a century ago by Marx. The task required avoidance of two equally dangerous snares, that of irrationalist reaction, on the one hand, and of

bourgeois positivism on the other. The effort to stay clear of these familiar traps, and also to breathe new life into an increasingly dogmatic and politically manipulated Marxism, left the Frankfurt School in an isolated forward position out of touch with nearly all segments of the activist intelligentsia. The need to unite theory and *praxis* could not be met, Jay notes, except on the doubtful understanding that theorizing itself might constitute action, "the only form of *praxis* still open to honest men."

In the end, as Jay suggests and as his mentor Hughes concludes in *The Sea Change*, the Frankfurt School leaned perceptibly closer to its sources in both early and late German romanticism than to the ossified Marxism of the parties or to the various schools of Western bourgeois thought. Its recent offshoot, the countercultural utopianism of Marcuse, which falls outside the time frame of Jay's book, was an authentic harking-back to some of the deepest hopes of romantic idealism. In any event, what the Frankfurt School set out to accomplish could not be done without strenuous intellectual and imaginative enterprise, in a sphere necessarily far removed from the exigencies of mass politics, and no less remote from the conventions of modern social science.

It is regrettable that Jay did not carry his history down to 1970, something he could easily have managed in two or three more chapters. But what he has written is superb. Sensitive to nuances of culture and personality, firmly grounded in the sources of modern intellectual history, and fortified by meticulous research that included numerous interviews and exchanges of letters, his work will stand for years. Jay approaches his thinkers with all due respect, but also with the "critical edge" they prized so much themselves. They could scarcely have found a better interpreter.

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ROBERT A. ISAAK. *Individuals and World Politics*. North Scituate, Mass.: Duxbury Press. 1975. Pp. xiv, 322.

This interesting "primer on world politics" finds that most thoughtful approaches to international relations focus either on the nation-state or on global systems. Asserting that both are necessarily abstract and stimulate a belief in man's powerlessness, this account affiliates itself with another major view, "the idealistic humanistic" one (p. 17). Choosing eight individuals who have prominently affected world politics in this century (Gandhi, Lenin, Mao, Woodrow Wilson, Hitler, de

Gaulle, Hammarskjöld, and Kissinger), the author presents for each a political biography of a dozen or more pages, immediately followed by about half a dozen pages on principles of world politics and several more on present implications. Into these various sections are infiltrated photographic portraits and a host of semi-isolated paragraphs that contain related comments, quotations, or definitions.

The author very effectively conveys the essence of foreign policy in the historic and contemporary state systems. He gives high marks to Kissinger as a scholar, as a pragmatic negotiator, and as the United States' greatest secretary of state. We hear of his devotion to the principle of indeterminacy, his weakness in economics, and his adoption of a blend of Metternich's emphases on balance and legitimacy and Bismarck's world view and flexibility. Despite the impressive skills of men like Kissinger, the author sees a future of "recession, depression, terrorism, starvation and war," a time when "the gap between developed and undeveloping countries will widen to ghastly proportions" (p. 257).

There are some disconcerting slips, like a reference to Mao's birth in the "village of Changsha." (Changsha is a city, and Mao was born elsewhere.) Wilson is set in opposition to Hitler: one is presented as a pathological idealist; the other, a pathological realist. Each was presumably characterized by extreme rigidity and dogmatism, characteristics that fit Wilson consistently only when he was left an invalid by his stroke in 1919.

The writing at its best is clear, interesting, and careful in defining terms. At its worst, it runs to jumbled figures of speech: "They knew how to make their political dreams into flesh by seizing the moment that was historically and psychologically ripe" (p. 263).

The test of a textbook lies in its meaningful use by students. If international relations can be taught in the present chaos of American urban high schools, this book will serve well; it is also appropriate for community colleges and for university courses at the freshmen level. It is not really a research effort, but it does move us a few steps toward a new mode of political realism.

EDWARD GULICK
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ANDREW W. CORDIER and WILDER FOOTE, selected and edited with commentary by. *Public Papers of the Secretaries-General of the United Nations*. Volume 4. *Dag Hammarskjöld, 1958-1960*. New York: Columbia University Press. 1974. Pp. xiv, 639. \$22.50.

ANDREW W. CORDIER and WILDER FOOTE, selected and edited with commentary by. *Public Papers of the*

Secretaries-General of the United Nations. Volume 5. *Dag Hammarskjöld, 1960-1961*. New York: Columbia University Press. 1975. Pp. xv, 592. \$27.50.

These two volumes, which are in a series intended by the editors to include texts that are "essential or most likely to be valuable to historical research" (vol. 1, p. vi), cover an important part of the period when the United Nations and the office of secretary-general under Dag Hammarskjöld achieved their greatest influence and prestige. High hopes were only to be dashed on the rocks of great-power rivalries.

These two volumes contain important texts not included in the official records of the United Nations. Among these are addresses of the secretary-general to groups outside the United Nations, statements to the press, and press conference transcripts. In the case of Hammarskjöld, the press conference transcripts are particularly valuable for illuminating his conception of the role of the United Nations and of the office he occupied. While these materials are available in various places, it is a great convenience to have them collected in one publication.

There is less value in reproducing texts from the official records because these are more likely concerned with specific UN activities, and the significance of particular reports or interventions by the secretary-general cannot be fully appreciated except in the context of what precedes or follows. Thus, the researcher must go to the official records of the Security Council or to those of the General Assembly if the secretary-general's statement was made in response to a criticism of his conduct or as an explanation of his execution of an Assembly resolution. To some extent the necessary context is provided by the commentary of the editors, but this can be adequate only for general informational purposes.

The commentaries, however, serve another purpose. Since Andrew W. Cordier was intimately involved in the work of the UN as executive assistant to the secretary-general and occasionally was his personal representative, the commentaries are useful for establishing the sequence of events and the motives and purposes of the principal actors. The editors' account of Cordier's closing of the Congolese airports and the radio station at Leopoldville in early September 1960 can be compared with that given by Brian Urquhart in his *Hammarskjöld* (1972). The importance of this action cannot be overemphasized because one consequence was the Soviet attack upon Hammarskjöld that threatened to destroy the usefulness of the office of the secretary-general. Urquhart reports that Hammarskjöld was "taken aback" when he heard of Cordier's action and implies that he would not

have approved such drastic action if he had been consulted in advance, though he subsequently gave full support to Cordier and took responsibility for his action in the Security Council and the General Assembly. The editors' commentary strongly defends Cordier's action as consistent with Hammarskjöld's instructions and fully justified by the situation in the Congo. Unfortunately, Cordier died before he was able to complete and publish a more detailed justification of his action.

The two volumes under review, together with volumes 2 and 3 covering the years 1953-58, make a valuable contribution to our understanding of the role of the United Nations and the part played by an imaginative, resourceful, and diplomatically skilled secretary-general in dealing with important issues of the time. They are probably most valuable for helping us understand Hammarskjöld's conception of the role of the United Nations and the part the secretary-general should play. Since one of the editors was at the same time an important and active participant in the action, the commentary of the editors assumes special importance.

LELAND M. GOODRICH
Columbia University

SAUL FRIEDLÄNDER. *Histoire et psychoanalyse: Essai sur les possibilités et les limites de la psychohistoire*. (L'Univers Historique.) Paris: Editions du Seuil. 1975. Pp. 233.

This work by one of the leading European psychohistorians is particularly welcome because it presents for the first time a coherent summary of the extant literature of psychohistory and particularly the current European contributions to this growing field. Friedländer attempts to assess what psychoanalysis can and cannot do for the historian, and what the psychohistorian should and should not research.

For Friedländer psychoanalysis is the psychological theory of choice for the historian because it is a historical theory of development, enabling history and psychoanalysis to pursue parallel and integrated lines of inquiry. Both disciplines always return to their own mode of validation. Psychoanalysis deals with time processes; it is evolutionary in the same way that history is. Psychoanalysis is also concerned with the complexity of emotions and motivations, while behavioral psychology is not, making psychoanalysis applicable to a wider range of complex historical phenomena than any other theory.

Among the areas where the field of psychohistorical inquiry lies open are the structure of the family, the problem of creativity in biography,

deviant patterns within a society, the formation of temporary groups, the choice of leaders, the personality changes that people undergo in group formation, and the deep symbolic structures of myth, conflict, and language in cultures.

There are also areas that Friedländer would exclude from the psychohistorical domain of study, such as social change, internalization of norms (object relations), and adaptation (ego psychology). This is the least arguable point in Friedländer's prescription for the future of psychohistory. If psychoanalytic theory is of value to historians, then its two current clinical developments, ego psychology and object relations theory, must also be assimilated and utilized by psychohistorians. Since history is in fact concerned with both continuity and change, and one element of both forces is internalization, the psychological how, when, and why of introjections and identifications are of capital importance to historians. It is times of personal and historical crisis that reveal most about underlying structures of tension and conflict. There has never been a time in history where social change was absent. It is a matter of assessing the strength and tempo of the impulses and resistances at work, and for this an understanding of ego energies of growth, defense, and adaptation is essential. While Friedländer's self-limitation upon psychohistory may in other cases be commendable, in the instance of ego psychology and object relations he forfeits current psychoanalytic approaches of great potential value to historical studies.

Friedländer holds that a psychohistorian, in order to work effectively, should be psychoanalyzed himself. The unanalyzed historian risks superficiality and mechanical application of psychoanalysis to history because he has a lower consciousness of his own intellectual processes. But, says Friedländer, this raises a paradoxical difficulty because psychoanalysis is a process that profoundly engages the whole person emotionally. This may mean an inability of the psychohistorian to exercise a critical spirit toward psychoanalysis. About this apparent paradox two things may be said. The first is that all historians have methodological and personal unconscious predispositions that cloud their perceptions in certain areas. The second is that psychoanalysis presumes to work through major emotional resistances to insight in an analysand, so that he will be particularly aware of his propensities to distort or overlook evidence or to become so emotionally involved with a given position that he can see nothing else.

Friedländer's notes are judicious and the bibliography of this rapidly growing field is excellent. The work is a must for historians who wish to keep abreast of methodological developments in psycho-

history and of current historical thought in the world.

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EDGAR WESLEY OWEN. *Trek of the Oil Finders: A History of Exploration for Petroleum*. (Semicentennial Commemorative Volume, memoir 6.) Tulsa: The American Association of Petroleum Geologists. 1975. Pp. xv, 1,647.

This book is one of three volumes authorized in 1963 by the executive committee of the American Association of Petroleum Geologists to commemorate the association's semicentennial. From the point of view of content and historical workmanship, Edgar Owen has produced a monumental book. It is a stout volume with 1,592 pages of text and double columns of printing on every page. The book is dedicated to Everette Lee DeGolyer, one of America's distinguished geologists and oil company executives. The emphasis is upon the evolution of petroleum geology as a science, theories as to where petroleum might be found, the establishment of geology departments by oil companies, the use of geology and geophysics in finding oil fields all over the world, the men who emerged as petroleum geologists and their particular contribution to the new and developing science in the United States and abroad, and the events from ancient times to the present that led to the finding of new oil fields. The author's knowledge of petroleum geology and petroleum geologists and his grasp of the literature is masterful. There are many data on the oil industry apart from geology—the history of different oil fields, leasing, drilling practices, the formation of oil companies that grew into major companies, oil company operations, the rise of international oil industry, conservation, the nationalization of the oil industry abroad, and the corporate structure of the petroleum industry. Except for the introduction written by Wallace E. Pratt, the chapter on Latin America by J. Herbert Sawyer, and brief sections on the different countries in Africa, the author has written the entire book. It is comprehensive in scope; there are twenty-four chapters; sixteen are about petroleum geology and the role of petroleum geologists in the United States; the remaining chapters are about oil fields in foreign countries.

The book is based on an enormous amount of research in oil company histories, articles on oil, mining and geological journals, the writings of petroleum geologists, state and foreign geological surveys, the U.S. Geological Surveys, special reports of petroleum geologists, old and rare books on petroleum, state and federal documents, plus

material that was derived from the author's "own business relationships, committee assignments and informal contacts during 57 years of professional work as a petroleum geologist, landman and participant" in some producing operations. The text is extensively documented, and at the end of each chapter there is a lengthy list of references cited in the text. Scattered throughout the book are many quotations from original sources. There are many maps of oil and gas fields as well as a subject, proper name, and geographical index. The book will serve as an invaluable reference work on the history of exploration for petroleum all over the world, petroleum geology, and petroleum geologists.

PAUL H. GIDDENS
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EDWARD SHORTER. *The Making of the Modern Family*. New York: Basic Books. 1975. Pp. 369. \$15.00.

CHARLES E. ROSENBERG, editor. *The Family in History*. (Haney Foundation Series, 17. The First Stephen Allen Kaplan Memorial Symposium in Social History.) Philadelphia: University of Pennsylvania Press. 1975. Pp. 210. \$10.00.

Over a decade ago Phillipe Ariès asserted that the nuclear family became the chief focus of sentiment, at the expense of extended family and community, only in the early modern period. Some historians are still influenced by this hypothesis, but there is growing evidence against it. Diane Hughes, for example, in her excellent contribution to the Rosenberg book, has shown that in medieval Genoa the nuclear family was the sentimental focus for artisans, though not for aristocrats.

Edward Shorter writes with charm about this subject, and many of his suggestions are plausible. He tends, however, to insinuate his case rather than to build it systematically. What we are offered is a provocative proposition in the guise of a serious proposal. I wish that Shorter had taken longer to collect his evidence.

Shorter's main thesis is that there was a great psychological "withdrawal" of the nuclear family from the surrounding community among the middle and lower classes of Europe after 1750. In support of this thesis he presents little direct evidence; what he does present is mixed with evidence to the contrary. He does not point to a particular community and show which families withdrew, to what extent, and when. Without having demonstrated that a significant change occurred, he jumps to an explanation of how it occurred. Shorter attributes the "withdrawal" of the nuclear family to a "revolution in sentiment." This revolution, he contends, was manifest in a more

affectionate relationship between husband and wife, who tended to marry for love, not money and offspring. "Popular marriage in former centuries was usually affectionless," he declares, using anecdotal evidence. An equal number of contrary anecdotes could be cited (for example, in Richard Gough, *History of Myddle* [1700]). Shorter has neither proven that marriage "for love" is more modern than traditional, nor that marriage "for convenience" is more traditional than modern.

In dealing with the modern period, Shorter convincingly describes declining community surveillance over courtship and marriage, permitting increased sexual freedom. Having presented statistics on the increase in illegitimate offspring born to urban working women, he suggests that these women "wished to be free" and were seeking "individual self-fulfillment." Scott and Tilly, in their contribution to the Rosenberg volume, show that such women were not liberated, nor did they wish to be; they just wanted to get married. Shorter, by inferring the motivation for sexual intercourse from one of its consequences, has committed a "phallacy."

Turning from the man-woman relationship to the mother-child relationship, Shorter sees a "revolution in sentiment" in the form of improved mothering. "Good mothering," he declares, "is an invention of modernization." His evidence is mainly that infanticide was widely practiced in early modern, but not industrial, Europe, and that the practice of giving an infant to a wet nurse in the countryside tended to disappear after 1750. No doubt there were shocking cases of child neglect and abuse in early modern Europe, as Shorter points out, but currently one million such cases are reported in the United States each year. No doubt the abandonment of newborn infants was widespread in early modern Europe, but how does the number of abandoned infants then compare with the number of unborn aborted now? No doubt there was a high mortality rate among infants given over to rural wet nurses, but how did this rate compare with that of infants reared in disease-ridden towns and fed with unsterile animal milk? When working women began to care for their own infants, around 1900, it was not because of a revolution in sentiment but in biological science.

Lawrence Stone, in his well-organized contribution to the Rosenberg book, has divided the history of the English family in the early modern period into two phases. He says that in the first, the sixteenth and early seventeenth centuries, the nuclear family became more patriarchal; in the second, the late seventeenth and eighteenth centuries, it became more affectionate. Considering only the first phase, Stone reports that the authority of the father was strengthened by laws, the legal rights of

wives were reduced, and children were subjected to harsh physical discipline. Stone thinks that this rather unattractive kind of nuclear family "rose" while the extended family declined.

His evidence, however, is not quite convincing. The fact that the state took over many clan functions does not tell us anything about how people in the clan felt about each other. Stone has not pointed out a particular clan and shown that nuclear families withdrew from it, to what extent, and when. His method is to cite prescriptive sources, such as laws and domestic advice books, illustrating them with anecdotes from actual family life. Illustration is not proof: for every anecdote about a patriarchal marriage in early modern England, one could find an anecdote about an egalitarian or matriarchal marriage. Stone's article is nevertheless worth reading.

In other contributions to the Rosenberg book, Wolfram Eberhard competently surveys upper-class Chinese family history; David Landes compares two Jewish financial families, but fails to enter into the psychosocial interior of either; and Michael Zuckerman takes Dr. Benjamin Spock to task for neglecting to build character and conscience in children.

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RUDOLF A. MAKKEEL. *Dilthey: Philosopher of the Human Studies*. Princeton: Princeton University Press. 1975. Pp. xiv, 456. Cloth \$20.00, paper \$9.50.

Dilthey's importance in the theory and history of literature has been widely acknowledged, but his contributions to the social sciences and psychology have been insufficiently explored. This situation is caused in part by his image as a humanist who was not interested in historical explanation, but also because of the dominant position of Max Weber as the theorist of a humanistic sociology. As a result, hardly any of Dilthey's writings have been translated into English. In this study Rudolf A. Makkeel points out some interesting aspects of Dilthey's approach to history. His focus is on Dilthey's psychology and esthetics, which are intimately linked with and which influenced his conception of the study of history. Arguing that, in contrast to the prevalent view, no break exists between Dilthey's earlier psychological and his later hermeneutic works, the author demonstrates the continuity in Dilthey's thought on the basis of his writings on esthetics. Literature was, in fact, Dilthey's test discipline for the human sciences. The link between esthetics and history is Dilthey's conception of the imagination: the poet and the

historian both attempted to bring out the typical in their treatments of reality. Their imagination was not arbitrary or fantastic but reality-bound.

The author characterizes Dilthey's goal in devising the human sciences as an attempt to develop an "aesthetic of history" that would fulfill the same function for the study of history as Kant's transcendental esthetic had for that of nature. In supporting his thesis, Makkreel utilizes Dilthey's writings on psychology, esthetics, and history, and he gives an insight into Dilthey's thought as a whole. In his reinterpretation Makkreel dispells some common misunderstandings, such as the notion that Dilthey's treatment of history is psychological or that empathy was the dominant conception in his approach to the past. Moreover, Makkreel does not consider Dilthey as a precursor or follower of other thinkers (Husserl, Heidegger, Kant, Hegel), but as a philosopher in his own right whose esthetics of history is relevant for the present. (In the last chapters the author gives some indications of his own ideas about an esthetics of history.)

Makkreel treats Dilthey's philosophy from a purely internalistic point of view: no reference is made to his position in society, his political opinions, or the ideological function his works might have had. Pointing out similarities and differences of Dilthey's thought in relation to other philosophers, and, to a lesser extent, psychologists, Makkreel does not include social scientists in his reflections.

The author presents his views by examining Dilthey's writings chronologically rather than thematically. By analyzing Dilthey's shifting views of such topics as psychology, esthetics, and history, which are evident in the various stages of his thought, Makkreel has made it somewhat difficult for the reader to follow his main line of argument. He compares Dilthey's thought with that of other thinkers of the period and with contemporaries' comments on topics Dilthey touched upon. These observations, however illuminating in themselves, further complicate the book.

Makkreel's study is nevertheless an important contribution to our understanding of Dilthey and the critical philosophy of history.

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FREDERICK M. SALLAGAR. *The Road to Total War*. New York: Van Nostrand Reinhold. 1975. Pp. xi, 197. \$11.95.

This book, originally a study written under the sponsorship of the RAND Corporation for the

United States Department of the Air Force, sets out to solve a theoretical problem in a real way, or a real problem theoretically. One can reasonably start at either end, for the thesis running through it is that total or all-out war is both by definition and fact of World War II origin. The attempt to give total war a dictionary definition and then to correlate warfare to it is itself a theoretical undertaking without any empirical value. It is, realistically, of no effect, except to condemn or condone real warfare situations.

Quite apart from that, this book confines itself almost exclusively to air warfare, fitting total or all-out war to that branch, and more particularly to its bomber subarm as it was employed in World War II. This is indeed a very serious weakness from the historical point of view, for, lacking precise definition, all-out or total war has not been unknown before. Not even the 1975-76 civil war in Lebanon, which did not have a single strategic or area bomber taking part, can be written off as being totally without an all-out or total war property. This book also fails to convince students of the art, science, and sociology of war and warfare that all-out or total war is synonymous with "indiscriminate" and "unrestrained," words the author uses repeatedly and perhaps even a bit indiscriminately.

To say, as the author does (p. 136), that the British wartime prime minister and Germany's chancellor had much in common in principle on the purpose and quality of war in general and the specific causes and purposes of World War II in particular seems not only unwarranted, but unnecessary and polemically distasteful as well. The apposition is placed this way: "Churchill and Hitler, although they held unprecedented power in their respective countries were also the victims . . . of events they could not control." As to what accounts for the author's, and the RAND Corporation's, neglect of President Roosevelt, I cannot say. The road to total or all-out war (which does not have its origin in the late war or in strategic area bombing as such)—that is, the worldwide scale of that war and the profound depth of the issues involved—can be historically as honorably linked to the president of the United States as to Churchill.

As near as I can judge from the bibliography, all twenty-eight titles listed are secondary works. The truly significant ones, both for the history of the war in all its facets, stages, and decisions and for interpretation or, better still, understanding, are the official histories of both the British and American governments. What is, therefore, wrong with this book originates in inadequate sources from which weak inferences are drawn.

In sum, deducing consequences from view-

points, despite a reliance on some select events and statesmen's statements, is bound to lead to meager results.

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ANCIENT

SARAH B. POMEROY. *Goddesses, Whores, Wives, and Slaves: Women in Classical Antiquity*. New York: Schocken Books. 1975. Pp. xiii, 265. \$8.95.

Despite the title, a marketing gimmick of the publisher, there is nothing low-brow about this work. Using evidence from legal transactions, inscriptions, papyri, pottery, literature, and historical records, as well as insights drawn from sociological, psychoanalytical, and quantitative approaches to history, Sarah Pomeroy has fulfilled the need for an up-to-date, carefully documented reference work and textbook on women in antiquity.

The author is sympathetic to women in a variety of societal situations. While the book begins with Greek goddesses and ends with the cult of Isis of Rome, it focuses on the private and public lives of Greek and Roman women. In fact, the goddesses themselves are viewed as married and single female members of the godly social structure. While Pomeroy cautiously avoids making an overall thesis, certain generalizations pervade the book. The independence of women during Sparta's wars, the Peloponnesian War, and the Second Punic War supports an assertion often made for modern history that women benefit during war. Likewise, unmarried goddesses, vestal virgins, and Roman widows indicate that "the most liberated females are those who are not bound to males in a permanent relationship" (p. 213). However, permanent relationships also receive recognition: of ancient Athenians, "we know of some courtesans who attempted to live as respectable wives, while we know of no citizen wives who wished to be courtesans" (p. 92). The relative happiness of the various groups she considers beyond a historian's knowledge, and the author reserves her feminist stand for the most striking of issues, the practice of female infanticide.

Pomeroy distrusts the use of literature to proclaim a prehistorical matriarchy or to provide evidence for powerful, unsecluded women in Hellenic Athens. Growth of upper-class, female economic and political power she sees as a feature of Hellenistic and of later Roman society. Her concern for understanding the lives of lower-class women is fulfilled best in her study of Roman slaves and workers. She testifies to the desire of freed men and women to purchase children born in bondage and

to the inequality of the Roman charity system—the much-praised dole was given only to men.

The greatest strength of the book is the clear presentation of the extant data and the Socratic avowal of areas of collective historical ignorance.

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JOSEPH VOGT. *Ancient Slavery and the Ideal of Man*. Translated by THOMAS WIEDEMANN. Cambridge, Mass.: Harvard University Press. 1975. Pp. x, 227. \$12.50.

Joseph Vogt has been interested in ancient slavery for many years. Under the auspices of the Mainz Academy he has done valuable work organizing studies in this subject. This volume presents an English translation of his collected essays, published previously in Germany under the title *Sklaverei und Humanität* (1965).

The edifying title of the book may ensure its sale in this country, but the quality of the essays is disappointing. Vogt seems to take for granted that Greeks and Romans became less tolerant of slavery as time went on and that philosophic and religious teaching must have affected their general attitude toward slavery. This is surprising, for he is well aware that neither Stoicism nor early Christianity sought to abolish it. In fact, Greek and Roman literature shows that the institution of slavery was accepted as a matter of course. When Vogt says that Aristotle justified slavery, he is misrepresenting what Aristotle said, and anyone who has read and understood the first book of *Politics* would recognize the misrepresentation. He is also misrepresenting Euripides when he uses passages from his plays as evidence that people in the fifth century were becoming increasingly sensitive to the evils of slavery. There are similar passages in Homer. Vogt does not seem to realize that while Athenians in the theater might have been thinking of their own kindred who suffered enslavement when they fell into enemy hands, they might not have been inclined to extend their sympathies toward non-Greek slaves in their own service. Many an Athenian, indeed, might have thought that Thracians or Phrygians should have been grateful for the privilege of living as a slave in Athens instead of in the "underworld" of their own country where true freedom was unknown.

Vogt has written his first essay, "Slavery and the Ideal of Man in Classical Greece," without taking into account the information found in the Attic orators, and this essay, like that which follows ("Slavery in Greek Utopias"), shows a curious lack of imagination. The Greek attitude toward slavery was conditioned by the exclusive character

of the Greek city state, and Greek philosophers were much more concerned with the good of citizens of their ideal states than with applying principles of justice to their slaves. Instead of offering essays on slavery in Hellenistic times, Vogt moves on to discuss the slave wars of the late Roman Republic. Here, too, when he asks why serious rebellions occurred only in certain places and at certain times, he seems more concerned with grievances than with the conditions that made united action by slaves possible. His belief that the appeal of Eunus was partly national, that he tried to present himself as a kind of Seleucid king, will not convince many readers. Other essays dealing with Roman slavery are well written and present some of the abundant evidence that Romans were often kind and considerate to their slaves, though it is surprising to find so little attention paid to Cicero's letters.

The student of ancient history will find interesting material in this book, but he should be warned against uncritical acceptance of its conclusions.

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JEREMY L. TOBEY. *The History of Ideas: A Bibliographical Introduction*. Volume 1, *Classical Antiquity*. Santa Barbara: Clio Press. 1975. Pp. xii, 211. \$17.25.

The present volume on classical antiquity is to be followed by three others, bringing the bibliographical introduction down to the present. The ideas to be considered are defined as those "used by educated people in each era to comprehend the universe and rationalize their social institutions, arts, and religion." Such ideas are dealt with under four main headings, to each of which a chapter is assigned: philosophy, science, esthetics, and religious thought in antiquity. These are preceded by a twenty-page introduction to the work in general and to this volume in particular; they are followed by a postscript, summarizing conclusions and pointing ahead to the next volume. There is also an author index to the modern literature and a table of contents sufficiently detailed for ready reference to the topics treated. The book is well arranged and clearly presented, and it should be useful to the intelligent layman for whom it is intended.

The author makes no pretense of neutrality: he is an adherent of the views of Arthur Lovejoy. He writes that "the works to be discussed are of two kinds: those that are recommended and others, well-known, that have a faulty approach to the history of ideas." But the discussion is perfunctory. A basic assumption is that the Greeks

were the intellectual force in antiquity, eclipsing the Babylonians and Egyptians who preceded and the Romans who followed them. This explains the lack of attention to Rome, especially republican Rome, and the emphasis on science and philosophy, while the humanitarian influence of Roman law is ignored.

There are serious omissions even within Tobey's framework. For example, a writer like Xenophon is simply ignored, yet he was universally read; while not an original thinker he is perhaps the best representative of the varied intellectual interests of the age in which he lived. Another strange omission concerns the field of geography. From an interest in geography came the debates over the relationship between man and his environment as well as more specifically oriented questions such as the rise of the Nile or the theory about the zones. The treatment of mathematics is excellent, however, and Tobey deals admirably with medicine. Space prevents further discussion here of religion and esthetics.

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HANS DELBRÜCK. *History of the Art of War within the Framework of Political History*. Volume 1, *Antiquity*. Translated from the German by WALTER J. RENFROE, JR. (Contributions in Military History, number 9.) Westport, Conn.: Greenwood Press. 1975. Pp. 604. \$25.00.

This English version of the first volume, published in 1900, of Hans Delbrück's grandiose effort to trace the strand of war through the web of universal history is based on the third revised edition of 1920. It is a conscientious, intelligent rendering that sticks closely to the German, but at the price of reading like a translation. The ease, elegant precision, and irony that distinguish the original are almost entirely missing. More serious is the absence of an introduction providing the historiographical context for Delbrück's criticism of classical authors and for his efforts to reconstruct the battles and military institutions of antiquity on the basis of objective criteria—*Sachkritik*. More than five decades have passed since Delbrück's last revisions; in the intervening years our understanding of Greek and Roman history has deepened, many of Delbrück's hypotheses have gained acceptance, and others have been disproved. To pass in silence over the state of classical studies that stimulated Delbrück's work, and to say nothing about the sizable literature that followed its appearance, is to do a disservice to the modern reader. If a new introduction was not considered necessary, the publishers might at least have in-

cluded a translation of Karl Christ's excellent reappraisal that precedes the text of the fourth German edition of 1964. The remaining volumes of Delbrück's *History* should be treated differently.

The continuing importance of this work lies less in its numerous reinterpretations than in the unusual, composite approach that led to them. From conditions of his own time, and from episodes of the early and later modern periods for which exhaustive documentation exists, Delbrück drew criteria—march speeds, logistic requirements, technical factors, demographic relationships—that permitted him to sort out the possible from the impossible in the classical tradition, and from which he interpreted the development of military institutions and strategic thought in antiquity. The dangers of schematic, rationalist explanations inherent in his extrapolations, comparisons, and quantification were usually avoided by the historicist outlook he brought to his work. Delbrück claimed scientific character for his methodology, and in some respects rightly so, but it was the humanistic scholarship and esthetic standards through which he filtered the findings of *Sachkritik* that gave his writings a value which justifies translation even at this late date.

PETER PARET
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SIGNE ISAGER and MOGENS HERMAN HANSEN. *Aspects of Athenian Society in the Fourth Century B.C.: A Historical Introduction to and Commentary on the Paragraphe Speeches and the Speech Against Dionysodorus in the Corpus Demosthenicum (XXXII–XXXVIII and LVI)*. Translated by JUDITH HSIANG ROSENMEIER. (Odense University Classical Studies, volume 5.) Odense: Odense University Press. 1975. Pp. 270.

The aspects of Athenian society spoken of in the title consist of treatments of Athenian population, imports and exports, maritime trade and loans, banking, mining, and the judicial process and procedure in private cases, with a detailed consideration of *paragraphe* and *diamartyria*, formal objections to bringing a case to trial (pp. 11–137). The authors include a commentary and suggested dating for eight Demosthenic speeches. They were chosen because the special plea, *paragraphe*, plays a part in them and because they concern the foreign trade of Athens that was a large (but relatively unnoticed) part of Athenian life (pp. 138–213).

The two parts are treated almost independently; there are few cross references. However, the subjects generally covered in the first part give the reader the setting for the situations involving the legal issues of the second.

The authors present completely and concisely,

with documentation, the pertinent factual data for the areas discussed. The interpretations of previous scholars are clearly reported and evaluated in a soundly critical manner. The whole could admirably serve as a syllabus for a student who wished to gain a full understanding of what is known on the subject, while the scholar will find the criticism, opinions, and conclusions of the authors not only stimulating but valuable in arriving at a more accurate interpretation of facts. For example, the observation that calculations of the population of Athens based on the production and import of grain (interpreted as wheat) are based on the incorrect assumption that Athenians did not eat barley.

The commentaries reconstruct as accurately as it is possible to do from the speeches what took place in each instance the part played by each character, the legal issues involved, and the use of *paragraphe* proceedings in the tactics of delaying a decision. Appendix 4 makes a good case for a distinction in meaning between *aphesis* and *appalage* against those who consider the terms to be a "synonym cluster" to denote a legal settlement by action of the plaintiff and/or the defendant. The three other appendixes present a chronology of Pasion's bank; maps of Athenian trade routes based on literary and epigraphical evidence, finds of Attic red-figured vases, coin hoards, and grave steles; and a summary of the evidence on *paragraphe* proceedings. A bibliography by subjects, a general index, and indexes of sources, geography, and names make the book eminently usable.

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R. J. A. TALBERT. *Timoleon and the Revival of Greek Sicily, 344–317 B.C.* (Cambridge Classical Studies.) New York: Cambridge University Press. 1975. Pp. xii, 234. \$15.00.

This is a comprehensive study of Timoleon's eight-year career in Sicily, his work of political liberation, constitutional reform, and economic revival that, as recent archeological excavations show, continued long after his political influence ended in 317 B.C. Talbert begins with two chapters on the ancient literary sources and then, in five subsequent chapters, examines the various chronological, military, political, and constitutional problems of Timoleon's career. The final three chapters deal with the relevant archeological and numismatic evidence. The special archeological bibliography, as well as the lists and charts in the numismatic chapters, will be particularly useful to the specialist.

Talbert's work is generally marked by caution, common sense, and a determination not to go beyond the evidence. He is willing to leave problems unresolved if the evidence proves inconclusive. For instance, on the events preceding Timoleon's capture of all Syracuse, Talbert concludes that no choice between the conflicting accounts of Plutarch and Diodorus is possible, having argued that Diodorus' version is not implausible and Plutarch's not without weaknesses. Similarly, he leaves open the possibility that Timoleon's treaty with Carthage guaranteed the independence of Greek cities in the Punic as well as the Greek zone of Sicily. On the constitutional reforms at Syracuse, Talbert believes that the pro-oligarchic Timoleon created a council with a limited membership, but insists that Diodorus' references to the Six Hundred neither support nor refute this opinion. He also rejects the idea that Plato influenced Timoleon in any way.

Talbert offers several other interesting suggestions and observations. He argues that Diodorus based his account of Timoleon chiefly on one unknown historian, but drew on Theopompus and possibly others for some details. The military studies point out Timoleon's remarkable abilities as a mercenary commander and the likelihood that his original force initially received little or no payment. In a later chapter he connects the fourth-century changes in Corinthian silver coinage and the almost simultaneous appearance of the new Corinthian Pegasi in Sicily with Timoleon's success in reviving agriculture and trade.

In sum, this is an excellent book, undoubtedly the best general treatment of Timoleon's career yet published.

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FRANK ADCOCK and D. J. MOSLEY. *Diplomacy in Ancient Greece*. (Aspects of Greek and Roman Life.) New York: St. Martin's Press, 1975. Pp. 287. \$17.95.

The Greeks, from their experience, did not have a name for diplomacy, nor did they develop a specialized vocabulary for its practice. The city states had no continuing institutions, such as a State Department, charged with the formulation and conduct of a definite foreign policy. Thus, although originating in a desire to maintain peaceful coexistence, international relations tended to depend on the ability of individuals attempting to implement policies formed hastily and under pressure. The governmental structures of the states also hampered policy making. In Sparta authority was divided between kings and ephors; in Athens a volatile

council and assembly made final decisions in open meeting.

The late Frank Adcock and D. J. Mosley use records of negotiations, meetings, and agreements of various types for their study. They present it in two parts. Before his death in 1968 Adcock composed a short narrative of Greek history to serve as a framework for the analysis of the forms and institutions of diplomacy contained in the second part. D. J. Mosley, known for his monograph, *Envoys and Diplomacy in Ancient Greece* (1973), prepared Adcock's work for publication and wrote the second, more lengthy, section. There is also an appendix containing a few selected documents in translation, a short bibliography, and notes on references to source material. There is virtually no discussion of problems of interpretation. The result is essentially a reference handbook with succinctly presented material and occasional penetrating comment, but with an overall treatment of Greek diplomacy that is disappointing.

Tersely and clearly written, Adcock's account establishes the framework of interstate relations for classical Greece from the formation of the Peloponnesian League through Philip of Macedon's League of Corinth. "a typical exercise of generous statesmanship and shrewd self-interest." But the author takes little more than token notice of the diplomatic activity of the Persian War period, which resulted in a successful war coalition, and of Hellenistic Greece. His interpretation is conventional and in some cases outdated. After a short survey of the evidence, Mosley discusses the aims of diplomacy, which was mainly concerned with the regulation of relations among the states and "in some respects a negative and defensive reaction rather than constructive." There is a good chapter on the methods of diplomacy, particularly in policy making, but the section on types of treaties is laborious. The concluding chapter, "The Instruments of Diplomacy," sketches the organization and working of Greek leagues, from the alliances of classical Greece through the federal systems of the Achaean and Aetolian Leagues of the Hellenistic period.

In short, Adcock's narrative is too brief to supply the historical context needed to vivify the analysis; Mosley's analysis is valuable for its collection and organization of material, but the general reader will find his section hard going because of the mass of detail.

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IOAN I. RUSSU, editor. *Introducere istorică și epigraphică, diplomele militare, tablitele cerate*. Preface by ȘTEFAN PASCU. (Inscripțiile Antice din Dacia și

Scythia Minor. First Series: Inscriptiile Daciei Romane, volume 1.) Bucharest: Academiei Republicii Socialiste România, 1975. Pp. 285.

Not since the late nineteenth-century publication of volume 3 (Dacia) of the *Corpus Inscriptionum Latinarum* (CIL), that monument of scholarly enterprise directed by Theodore Mommsen, has anyone assembled a new collection of inscriptions of the Roman province of Dacia (A.D. 106–271). Because of the numerous epigraphical discoveries made since then, especially over the last thirty years, and published in specialized and often obscure journals, an up-to-date *Corpus* would be an important contribution to further research and study of the history of Dacia. The task of making such a compilation, however, requires extraordinary linguistic skills and a vast technical knowledge. Widely recognized in Europe for his numerous books and articles dealing with Dacian inscriptions, Ioan I. Russu is uniquely qualified to undertake this assignment. The first volume is a superb epigraphical achievement worthy of its place as the successor of Mommsen's work.

Russu's new edition brings together all of the Dacian inscriptions known to date, including those published in the CIL, which in many cases are here revised and corrected. Besides a Romanian translation and pertinent bibliography, a photograph or drawing (often both) accompanies each inscription, thus offering an exact copy of the original text—a vast improvement over the CIL. In addition, Russu has assembled military diplomas and waxed tablets, leaving the remaining types of inscriptions (including *instrumenta*) for the second volume, as yet unpublished. The arrangement of documents in the first volume is chronological; in the second it will be geographical, from southwest to north, following Trajan's path of conquest. Either is a far more practical grouping than that found in the CIL, where the inscriptions are grouped generally from north to southwest, following the direction of Mommsen's travels in 1857 over the territory of old Dacia.

The first volume contains a brief but thorough and accurate account of the Roman conquest, organization, and development of the province (ch. 1). An explanation of the importance of inscriptions to the study of Dacian history and a survey of Dacian epigraphy follow (ch. 2). Of special note here is Russu's estimate of the valuable epigraphical contribution of the nineteenth-century scholar, Ștefan Moldovanu, whose work was omitted in Mommsen's study.

Scholars know of thirty military diplomas from Dacia, ranging in date from A.D. 86 to 164. Of these, twenty-three mention Roman auxiliary units stationed in Dacia, while seven mention units sta-

tioned in other provinces. Several diplomas are extremely fragmentary and are here published for the first time. Of particular value to Roman military history, these documents contain a wealth of information for Roman prosopography, as well as for political and social history.

Twenty-five waxed tablet inscriptions dating from A.D. 131 to 167, discovered in the eighteenth and nineteenth centuries in the gold mines of the Apusenian Mountains, comprise the final chapter. Russu's thorough treatment of these important economic documents completely replaces the discussion of them in the CIL.

Roman historians will long remain indebted to Russu for the excellence and usefulness of this publication. His promised second volume excites eager anticipation.

DONALD W. WADE
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PAUL MACKENDRICK. *The Dacian Stones Speak*. Chapel Hill: University of North Carolina Press, 1975. Pp. xxi, 248. \$12.95.

Paul MacKendrick's latest exercise in petrological ventriloquism describes the archeology of Romania from prehistoric to early Byzantine times. The subject is fascinating. Romanian archeology has provided important evidence on such diverse subjects as the spread of the Neolithic, interaction between Greek colonies and local tribes, the development of Dacian kingdoms, and the rapidity and depth of acculturation in the latest of Rome's provinces. The history of Romania, from its position as the chief Romance language state in Central Europe to its present position as a Marxist nation, has shaped archeology and made the study of the discipline an interesting aspect of intellectual history.

MacKendrick's book follows a familiar format started in 1960 with *The Mute Stones Speak*. The author concentrates on the classical period, but he attempts to place it against preclassical developments. The narrative is very site-oriented, with descriptions of many excavations illustrating different periods. These accounts are generally clear, although sometimes dry, and are accompanied by good plans and maps and passable photographs. Descriptions of sites are combined with some information on the excavators, although this is less abundant than in previous volumes. The anecdotes are amusing, but superficial, and add little to an appreciation of the development of Romanian archeology.

The book has several positive qualities. No other book in English covers prehistoric Greek, Dacian, and Roman material. The account is accurate and updated. The bibliography, maps, and chronological charts are useful. The most serious limitation is

the conceptual framework. MacKendrick conveys a trite vision of the ancient world, centering on the sublimity of Greek culture, the brutality of Roman imperial and class oppression, and the foibles of Roman emperors. The material itself often calls for more sophisticated interpretation. The frame of reference is largely limited to books by MacKendrick himself. In discussing the Neolithic, for example, he refers only to Greek archeology without any attempt to place the Romanian material in the larger context of northeast Mediterranean prehistory that has developed so rapidly in recent years. He conveys no sense that modern archeology can be used as a sophisticated social science tool. No real consideration is given to the various forces, from nationalism through fascism to Marxism, that have shaped Romanian archeology.

The scholar will be grateful to MacKendrick for this introductory work. Its limitations, however, should be kept in mind and interested people urged to explore more deeply the complex world of Romanian archeology.

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ANTHONY BIRLEY. *Septimius Severus: The African Emperor*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. xiv, 398. \$8.95.

P. A. L. GREENHALGH. *The Year of the Four Emperors*. New York: Barnes and Noble. 1975. Pp. xv, 271. \$17.00.

ROBIN SEAGER. *Tiberius*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xviii, 300. \$12.95.

Plutarch says that he wrote biography, not history. In his *Lives*, history is only a background against which he depicts the noble and base deeds of his heroes. But modern historians are different. Ronald Syme once remarked that "biography offers the easy approach to history." When a contemporary ancient historian professes to be writing biography he certainly will wind up writing his kind of history; when he says history, the bet is that what he really has in mind is prosopography. This is in keeping with another "Symean law" according to which "in all ages, whatever the form and name of government, be it monarchy, republic or democracy, an oligarchy lurks behind the façade; and Roman history, Republican or Imperial, is the history of the governing class" (Ronald Syme, *The Roman Revolution* [1939], p. 7). But the strange peculiarity of an authoritarian regime is that there also is a despot, cruel or benign. To find him one need not look behind the façade: he lurks everywhere.

The three books under review exemplify all the

virtues and shortcomings of the maxims "through biography to history" and "oligarchy first, despot next." P. A. L. Greenhalgh is a specialist in the history of early Greek warfare. *The Year of the Four Emperors*, his first book dealing with Roman history, is a popular and straightforward account of events and personalities of the fateful year 68-69, when after the death of Nero four emperors occupied the throne in succession. It is informative and readable, and probably nothing more needs to be said of it. The books by Seager and Birley are important scholarly monographs; they have footnotes, bibliographies, and, in the case of Birley's book, a detailed and very valuable prosopographical appendix. In matters of scholarly erudition both books are beyond reproach, as was to be expected of their authors. Robin Seager has made a name as a historian of the Roman Republic, and Anthony Birley has published extensively in the field of imperial history.

Seager's *Tiberius* is a rationalistically inclined gentleman—Seager would have none of his idiosyncrasies and follies because they are not of particular interest to him. Obviously they cannot be of any importance to a "sober" historian, who, like Seager, seriously maintains that "Tiberius' principal virtue was moderation" and that "the servility of senate and people gave him ample opportunity to exercise it" (p. 142). But "to preserve moderation in matters of honours was not easy" (p. 143), nor was it easy for Tiberius to maintain the appearance of republican freedom in his dealings with individual senators" (p. 132). Nor is it easy to believe in Tiberius' propaganda of *moderatio*, but Seager is somehow convinced. The principate was forced upon Tiberius against his will by Augustus (and the Senate). He tried to refuse it, and "to deny Tiberius' sincerity is perverse" (p. 56). Seager quotes Béranger's brilliant study on the ceremony of the refusal of power, but he has not learned too much from it. It is a misunderstanding to speak of sincerity or dissimulation. This ceremony was not a "polite comedy" but a religious, magical, and political ritual. In an authoritarian regime everything tends to be ritualized, and nothing is a farce.

Following in the footsteps of Augustus, Tiberius was, of course, also interested in raising the "moral" standards of society, sexual and otherwise. The old trick of despots and other benighted minds obviously works: Seager swallows the bait. Nor can Birley refrain from condemning the "levity" of women, stage, and arena (p. 101). Seager, following Suetonius, credits Tiberius also with sumptuary measures—"limiting the activities of food-shops and restaurants, and forbidding altogether the sale of pastries" (p. 141). Tiberius the republican loved freedom, but even he could not do too much about it. For security reasons some

people had to be "removed," but both authors agree that "counting of heads misses the point" (Seager, p. 239), and that "it is no good counting heads" (Birley, p. 279). Seager admits, however, that Tiberius' rule degenerated into a reign of terror (p. 239); Birley on the other hand defends Septimius Severus against the *obtrectatores* (ancient and modern) who called him "Punic Sulla." He was neither Punic (his accent was provincial) nor a cruel tyrant. His behavior was "much more normal than historians of Rome like to remember" (p. 281). He rates rather low in comparison with Vespasian (who executed few senators), but rather high if compared with Commodus or Domitian.

The trouble with Western historians of the Roman Empire is that they know too well the history of the republic and that they themselves are republicans who are more or less converted to the "necessity" of autocracy. If they knew better the history of China or Russia they would be much less perplexed. In an autocracy, purges, servility, and adulation are not vices but virtues, signs not of decadence but of political skill. For a historian of Imperial Rome Machiavelli and Wittfogel are as important as Tacitus and Syme.

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J. H. W. G. LIEBESCHUETZ. *Antioch: City and Imperial Administration in the Later Roman Empire*. New York: Oxford University Press, 1972. Pp. xiv, 302. \$17.00.

The Syrian city of Antioch in the fourth century A.D. has received its due, which is considerable, from ancient and modern authors. The voluminous writings of the Sophist Libanius have provided the foundation for many an edifice of modern scholarship. One thinks of Seeck's masterly study of the chronology of Libanius' letters, Petit's *Libanius et la vie municipale à Antioche au IV^e siècle après J.-C.*, Festugière's *Antioche païenne et chrétienne*, and the middle part of Downey's *History of Antioch in Syria*. Liebeschuetz adds to this with a social, economic, and administrative survey of the fourth-century city along the lines laid out by his teacher, A. H. M. Jones. The phrase "later Roman Empire" in the subtitle is misleading, but within its limits the book is useful for its abundant citations from the ancient evidence.

Liebeschuetz is strongest in his exploitation of literary sources—it is indicative that he chose to begin with a review of the life and works of Libanius—yet Petit's books and Norman's work on individual speeches still remain basic. Somehow in writing what is a work of synthesis Liebeschuetz never penetrates to the life of that teeming and turbulent city. A passing reference on page 134 to

the magnificent private residences at Antioch is insufficient for a topic to which the Princeton mosaics can contribute handsomely. Six pages under the forbidding rubric "Routine Entertainment" are all that we have of dancing, acting, chariot racing, and (a little oddly) bathing establishments. Liebeschuetz's treatment of Antioch's relations with the Syrian countryside is competent but again rather drab. A rich variety of items on Syrian hermits is assembled on page 234, but without any inspiration from Peter Brown's perceptive article on the rise and function of the holy man (*Journal of Roman Studies*, 61 [1971]: 80 ff.).

A study of Antioch must not depend only upon classical literary sources. It requires close attention to the geography of Syria, the topography of the city, and the results of the Antioch excavations. The absence of a plan of the ancient city and the inadequacy of the description of the site are symptomatic of a general weakness in Liebeschuetz's *Antioch*. Occasionally he makes good use of Tchalenko's pioneering *Villages antiques de la Syrie du nord*, as on pages 71–72 in refutation of Libanius, but one misses an overall awareness of the physical contours and remains. Unfamiliarity with archaeological evidence has also infected a map of the Syrian defense system: recent excavation has made it certain, for example, that Qasr el Heir, north-east of Palmyra, is not Adada as shown.

Finally, some trouble must be noted in the area of nonclassical literary sources. Bardaisan would have been surprised to learn that Christian Syriac literature at Edessa came into existence in the fourth century (p. 62).

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WALTER GOFFART. *Caput and Colonate: Towards a History of Late Roman Taxation*. (Phoenix: Journal of the Classical Association of Canada. Supplementary volume 12.) [Toronto:] University of Toronto Press, 1974. Pp. 165. \$10.00.

The peoples of the Roman Empire were also certain about death and taxes, but we have had better understanding of how they suffered the former than of how they exacted the latter. This excellent book now remedies the situation. Goffart shows that *tributum* was a contribution, not a direct tax based on census returns, though the census possibly determined the lump sum levied on provinces. The management, collection, and even apportionment of municipal taxes were in the hands of local aristocrats who not only paid the major share but also performed the *munera*, an essential part of imperial taxation and vital to the local communities. Liability was apportioned on the basis of

personal *professiones*, but how responsibility was shared by the lower classes remains a mystery.

After the currency collapse of the third century, Diocletian collected taxes in kind, the *annona*, from all parts (and more people) of the empire, but he continued to rely on municipal initiative, personal *professiones*, and local tax registers for its collection. Diocletian checked abuses by introducing abstract units of account, *iugum* and *caput*, into which all resources of the empire were reduced by means of a schematic survey. Both *iugum* and *caput* were shares of assessment, not land and head taxes, and tax liability was converted into shares of the assessment by means of uniform conversion schedules for lands and mortals. In a postscript Goffart argues that the rank and file of the military system served as Diocletian's model for this innovation.

Through a chronological analysis of pertinent laws, Goffart defines the changing meanings of tax terms and clearly demonstrates how personal *professiones* and local tax registers came to be used by the imperial government, almost like a census, until gradually *iugum* and *caput* became the basis for taxing lands and souls late in the fourth century. Finally, when land alone came to bear the tax burden, the *colonus* was bound to the soil to guarantee its productivity. While I cannot agree with all Goffart's political, social, and economic conclusions drawn from the methods of taxation outlined, I have reserved disagreement in order to focus on the book's very real merit. Ancient historians should pay close attention to the author's complicated but carefully reasoned arguments.

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MEDIEVAL

WILLIAM R. BEER, translator. *Slavery and Serfdom in the Middle Ages: Selected Essays by Marc Bloch*. (Publications of the Center for Medieval and Renaissance Studies, 8.) Berkeley and Los Angeles: University of California Press. 1975. Pp. ix, 276. \$17.50.

PETER CLEMOES, editor. *Anglo-Saxon England 4*. New York: Cambridge University Press. 1975. Pp. ix, 262. \$24.00.

A comprehensive and systematic history of medieval slavery and serfdom has yet to be written. Charles Verlinden's *L'Esclavage dans l'Europe Médiévale*, which appeared more than twenty years ago as the first of a projected four-volume study, is a monumental achievement. It contains a collection of documents essential for a history of slavery but deals with slavery only in the late Roman Empire in the West, the Iberian Peninsula, and

southern France. More recently, the chapter on the Middle Ages in David Brion Davis' ambitious work, *The Problem of Slavery in Western Culture*, is concerned with theories of slavery rather than with actual practice. Davis views Western culture from the limited vantage point of North America, and he was innocent of most of Marc Bloch's writings on slavery.

The volume under consideration contains six studies on medieval slavery and serfdom, all previously published but brought together here from often inaccessible journals and translated into English for the first time. In the classic essay, "How Ancient Slavery Came to an End," Bloch shows how the breakup of the Roman latifundia contributed to the decline of slavery in the empire and how trade and war perpetuated it. The attitudes of the Germanic laws, the dilemma of the Church, the varying meanings of the terms slave and serf, the methods and meanings of manumission, together with the economic necessities of the times are treated with respect to the growth of serfdom in the Carolingian period. The second article, "Personal Liberty and Servitude," continues the discussion and sketches in broad outline the evolution from slavery to serfdom in Western Europe down to about 1300. The other four are specialized studies, emphasizing the variety and complexity of serfdom where local custom determined particular conditions and obligations.

Beer has obviously struggled with Bloch's sometimes complicated syntax, and the resulting translation is generally smooth and lucid. The notes at the end of the book, apart from illustrating once again Bloch's proverbial learning, invite attention to the wealth of primary and secondary material that must be examined before a thorough treatment of the subject can be written. The recent researches of such scholars as Theodore Evergates and John Freed require modifications of Bloch's interpretations of feudal society, but until the study of medieval slavery and serfdom is finally produced, these writings of Marc Bloch remain the starting point.

Anglo-Saxon England 4 is the fourth volume in an annual series devoted completely to Anglo-Saxon studies by using "less commonly considered forms of evidence" and a cross-disciplinary approach. This important book both contributes to the realization of the editor's goals and sustains the high level of scholarship of its predecessors. Of the thirteen articles here, six deal with language and literature, two with art and crafts, two with history, and one each with liturgy and numismatics. The final paper is a list of the forms, symbols, and abbreviations used in the *Dictionary of Old English* and in *Old English Syntax* (both now in preparation); these forms and symbols should be used in

papers submitted to *Anglo-Saxon England*. One of the history papers is a valuable review article discussing all the literature on Scandinavian settlements in England published since the appearance of Stenton's *Anglo-Saxon England* in 1943. The bibliography listing publications for 1974 will be indispensable for students of many aspects of early English culture.

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HARRY W. HAZARD, editor. *A History of the Crusades. Volume 3, The Fourteenth and Fifteenth Centuries*. Madison: University of Wisconsin Press. 1975. Pp. xxi, 813. \$25.00.

This is the third large volume in the monumental, multiauthor history of the Crusades projected many years ago by A. C. Krey, Duncalf, and La-Monte and finally, but very slowly, seeing the light of day under the direction of Kenneth M. Setton and his collaborators. The editor of this volume, Harry W. Hazard, remarks that the book has been delayed for twenty years, so that some of the chapters were originally prepared in the 1950s and 1960s. Four of the authors have died. The long postponement has been unfortunate, but the volume is at last very welcome.

As in the previous two volumes, each author is a distinguished authority on his subject. Aziz S. Atiya leads off with a skillful survey of the Crusades of the fourteenth century. Deno Geanakoplos offers two chapters on Byzantium and the Crusades, focusing on the unsuccessful efforts of Eastern and Western Christendom to resolve their religious differences in the interest of presenting a united front in the defense of Byzantium against the infidel. Peter Topping presents two chapters on the struggles for power among dynastic factions in the Morea that are as learned as one would expect from him but rather dense with dynastic detail (for example, "the dower of Louis's consort Joanna, a granddaughter of John and Gravina and daughter of the late Marie of Anjou, sister of Queen Joanna"). Setton himself gives us a couple of chapters on the Catalan adventurers and their rivals in Greece. Anthony Luttrell tells of the futile crusading efforts of the Hospitalers, who were faced with insufficient resources, lack of powerful allies, the rivalries of Venetians and Genoese, Greeks and Latins, the ineffectiveness of papal crusading policy, and the general indifference of Latin Christendom. Ettore Rossi continues the tale of the Hospitalers after 1421, centering upon the defense and the ultimate loss of Rhodes. Sir Harry Luke makes an interesting narrative of the efforts of the Lusignans of Cyprus to carry on the Crusades, their

vassalage to the sultan after 1426, and the acquisition and loss of the island by Venice. Charles Julian Bishko recounts the *reconquista* from the late eleventh century, concentrating on the twelfth and thirteenth centuries and continuing through the fall of Granada. Hazard tells of the decline of Muslim North Africa. Another of the more readable chapters is that by Mustafa M. Ziada on the troubled successions to power in Mamluk Egypt and the unruliness of the Mamluk soldiery. Denis Sinor writes of Latin contacts with the Mongols and the shifting alliances of Byzantines, Latins, the Golden Horde, and the Ilkhans. Edgar N. Johnson recounts the expansion of the more advanced Germans in the lands on the southern and eastern shores of the Baltic at the expense of the more primitive Slavs. Frederick G. Heymann gives a very concentrated account of the futile efforts of empire and papacy to repress the Hussites, 1419-36, to the compromise at Basel, which enabled the movement to survive until its absorption in the sixteenth-century Reformation. Finally, we have another survey chapter by Atiya on the fifteenth-century Crusades.

Each chapter, except for Atiya's surveys, is provided with an initial bibliographical note, and there are a number of maps, a very helpful gazetteer, a chronology, and a good index.

The volume as a whole shares many characteristics with those other great collaborative works, the Cambridge histories. The authors are outstanding scholars, the subjects are treated relatively fully and authoritatively, and it is packed with one fact after another, largely concerning dynastic history. It will be invaluable to specialists and for reference use, but even professional medieval historians will find it difficult to read some of this generally unfamiliar material for general knowledge and pleasure. This is not meant to be highly critical, but merely descriptive. For the right uses, it will be a superb tool.

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EILEEN POWER. *Medieval Women*. Edited by M. M. POSTAN. New York: Cambridge University Press. 1976. Pp. 112. Cloth \$12.95, paper \$4.45.

This delightful little book brings together a number of essays about medieval women prepared by Eileen Power before her premature death in the 1940s. These studies have been selected and arranged by Power's husband and colleague, M. M. Postan, who also added the footnotes and brief bibliography.

Although not couched in a scholarly guise, since

most of these papers were originally prepared as popular lectures, these studies nonetheless offer a wide ranging and reliable discussion about the role of women in their various social classes, with examples drawn from England, France, and Germany. Since these papers were written before the opening of the feminist crusade in its current phase, they avoid the sometimes contentious note of recent work on the place of women in European society. In this work, women emerge as an essential component of the medieval social order; their significance was often much greater and more independent than their formal legal position may have implied.

From an opening chapter on medieval ideas about women—shown to be delightfully contradictory—through chapters on the lady, the working woman in town and country, the education of women, and nunneries, Power discusses medieval women in a charming and witty style.

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FREDERICK H. RUSSELL. *The Just War in the Middle Ages*. (Cambridge Studies in Medieval Life and Thought. Third Series, volume 8.) New York: Cambridge University Press. 1975. Pp. xi, 332. \$32.50.

The Just War in the Middle Ages is a distinguished book that is a worthy and valuable addition to the third series of Cambridge Studies in Medieval Life and Thought. Frederick Russell has utilized all the primary and secondary material available to him, including manuscripts and early printed books, to present an exhaustive study of medieval just war theories. He writes clearly and logically, and his book is certainly a definitive account that will be useful to scholars in many different fields.

Aristotle first used the term just war, and Russell sets the stage for his discussion of the medieval theories by a brief account of classical traditions, Old and New Testament attitudes, and Roman law precedents. St. Augustine, however, was the most seminal figure in the early Church in this area as in so many others, and Russell treats at length Augustine's view that war was "both a consequence of sin and a remedy for it." It was Augustine who legitimized warfare and made it necessary rather than inherently sinful by grounding it in evangelical precepts, especially the notion of charity—war, if carried on without malice, was an act of love, since it punished evildoers and prevented them from doing further wrong.

Russell next focuses on Gratian's *Decretum*, pointing out that it was Gratian who extended

divine authority for waging war to ecclesiastical authority and gave the Church a moral mandate to wage war in its own behalf. Gratian also advanced a legal definition of the just war and suggested conditions under which it should be carried out, thus laying the basis for the introduction of the concept into modern international jurisprudence.

The Decretists explained, consolidated, systemized, and updated the analysis of war in the *Decretum*, elucidating the purpose of warfare and restricting the locus of authority to wage war to "princes, popes, and prelates." By Huguccio's time, the use of violence was considered the monopoly of legitimate authority and justified whenever necessary to protect the patria or the Church from their various enemies. The Decretalists then accommodated previous ideas to the political conflicts of the thirteenth century, but with an emphasis less on the just cause than on questions of authority and jurisdiction.

A concluding chapter focuses on St. Thomas Aquinas and his circle, and Russell portrays Aquinas as a most traditional yet innovative thinker whose major contributions were his application of Aristotelian political convictions to the analysis of war and his attempt (although not in any systematic way) to synthesize Augustine's idea of war as punishment for sin with Aristotle's notions of the common good and *raison d'état*.

Russell's approach is basically textual, and he admits to tunnel vision in analyzing the materials. He does, however, make frequent reference to the context in which the various theories were being worked out. The Crusades, a specifically medieval development, receive no separate or extensive treatment in this book, although Russell makes many interesting observations about them and devotes several pages to the way the Decretalists, especially Innocent IV and Hostiensis, handled the matter. In general, the canonists and theologians hesitated to assimilate the Crusades into their just war theories, a reticence attributed to their reluctance to involve the Church too directly in bloodshed. As the author says, however, "The restrained justification of crusades and other ecclesiastical wars of course constituted no barrier to unrestrained prosecution of these wars" (p. 296).

In his conclusion, Russell calls attention to the time-bound nature of medieval just war theories and other deficiencies that preclude their general application in modern times. Yet his book, an important and scholarly contribution to medieval studies, is quite timely and raises provocative questions for our own age.

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JONATHAN SUMPTION. *Pilgrimage: An Image of Mediaeval Religion*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 391. \$15.00.

This industrious compilation of instructive, entertaining episodes and information from pilgrimage history disappoints at several levels. It is not the much-needed survey of pilgrimage history, nor really a probing of medieval popular religion under the aspect or microcosm of pilgrimage, but rather Jonathan Sumption's gloomy impressions of that religion as illustrated by a selection of episodes jumbled together from ten centuries and several countries.

Chapter topics fill roughly thirty-five pages on the cult of saints and relics, twenty on miracles and charlatanry, sixteen on cures and medicine, nine on origins and ideals, sixteen on pilgrimage as penance or penalty, twenty-two on the Great Age (from 1000), eight on the pilgrim's vow and indulgences, twenty on cult (canonization, publicity, offerings), forty-two on "The Journey" (which is fascinating, and the best part of the book), six on the shrine, forty on Rome, ten on pilgrimage as tourism, twenty on later medieval cult, and fourteen by way of summary.

In content, *Pilgrimage* views the thousand years of medieval popular religion as a pathology (its practitioners were a brutalized, gullible, death-and-devil obsessed *massa damnata*), with pilgrimage as an aberration of ritual magic to assuage claustrophobic guilt and terror. In methodology it is old-fashioned G. G. Coulton: juxtaposition of sources widely separated in time, place, and character, bereft of context or examination, each taken at face value. Even the pathology of popular religion is a valuable field, but it would demand wide erudition about the religion (its varied *Sitz im Leben*), a disciplined focus in time and place, and the newer methodologies instead of mere sifting for stories. Emmanuel Le Roy Ladurie has recently demonstrated what can be done here in his celebrated *Peasants of Languedoc*.

Platoons of doctoral students in the new medievalism, armed with the weapons of the Bloch-Febvre *Annales* school, should be deployed against the challenge of pilgrimage as image, as microcosm, as media and tourism, as pathology, and as deep human drive or tropism. Meanwhile the knowledgeable medievalist can extract much incidental profit by excising and transplanting data the author has laboriously gleaned.

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CHRISTOPHER N. L. BROOKE, assisted by GILLIAN KEIR. *London, 800-1216: The Shaping of a City*. His-

tory of London.) Berkeley and Los Angeles: University of California Press. 1975. Pp. xxi, 424. \$21.00.

It is trite to say that this is a big book worthy of its subject. In both conception and execution this is a major study, which will be used and cited for many years. The work reflects and justifies the years of effort the authors devoted to planning, writing, and traveling. It is one of those happy products that stand midway between monographic and synthetic work, and it faces, with value and dignity, in both directions.

The book begins with the London of Offa of Mercia and of Alfred the Great and runs through the death of John. We have by then the recognizable medieval map of the city, almost a full complement of the intramural regular houses and churches, a municipal charter, and a mayor. Since the main story only begins with the Norman Conquest, the choice of the earlier date is largely to illustrate the basis on which Norman London rested. By the time we take our farewell in 1216 we can look back upon FitzStephen's famous description of Plantagenet London, upon the turbulent movement for a commune, and into some identification and analysis of the new oligarchy that was to dominate the future. My only criticism is that, in an already long and complex book, there is a tendency to include items of little relevance. The offending portions are mainly those that deal with the city's religious houses. These sections are set in the context of the intellectual life of the twelfth century and only by inference in that of urban growth. Those parts covering Italian towns and the general problems of urban development, however, are very good, and full credit goes to the authors for their inclusion.

The development of a great city is a drama of social change and evolution. The dramatic tension in this version of one particular case study comes from the way several major themes are juxtaposed: self-government or royal control (the city's mayors versus the king's sheriffs), a city of Englishmen or of foreigners (Vikings, Normans, Flemings, or Frenchmen), and an insular city without its own archbishop or a European port and metropolis. The wonder is that there are no simple answers, no winners. The growth of wards and parishes, of the mayor's office, of suburbs, and of banks and the mint, are all separate but related incidents in the continuous drama. The urban revolution of the twelfth century was a great milestone in the emergence of modern European civilization. London had a reasonable role in this, as well as a major one in the growth of the English kingdom and people. Both aspects are well illuminated here.

The book, even by today's outrageous prices, represents good value for the money.

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ROBERT FOLZ. *The Coronation of Charlemagne, 25 December 800*. Translated by J. E. ANDERSON. Boston: Routledge and Kegan Paul. 1975. Pp. xii, 266. \$18.00.

First published in French in 1964, this small masterpiece is at long last available in English translation. Robert Folz is well known for his monumental studies of the legend and cult of Charlemagne in the Middle Ages; for this reason he is better qualified than most historians to convey a deep understanding of the efforts and significance of Charlemagne for France and Europe. Above all, even in his own lifetime Charlemagne towered, somewhat charismatically, in the imagination of his contemporaries. Folz's special sensitivity to the religious and spiritual nuances of Charlemagne's impact enables him to provide a thoughtful, scholarly, and humane portrait of the events leading up to the coronation in A.D. 800.

It is unfortunate that the publishers should have chosen for the dust jacket a late medieval, idealized portrait of Charlemagne, which misdirects the reader's attention. Folz certainly does not. He concentrates on the growth of Frankish power, starting with Clovis and leading to Charlemagne. As the topic requires, the focus is on political and religious developments that explain the imperial coronation, rather than on social and economic history. The finest chapter of the book, "The Empire in Theory and Practice," surveys the immediate consequences of the coronation—the new outlook and sense of responsibility it promoted. Folz presents all this with the brevity and lucidity that prolonged occupation with the subject makes possible; the elegance of style reflects his easy familiarity with the source material.

I hesitate to criticize a good book, but when I put it down I felt the author had not touched upon those nagging doubts that keep disturbing my understanding of the reign of Charlemagne. The emperor dominates the preceding and succeeding centuries—or so the sources make us believe. But we know that the technology available and the social and geographical impediments to administration must have made it impossible for Charlemagne to translate into practice the politico-religious conceptions that inspired his laws and letters. What are we to make of this contradiction? Folz does not address himself to this question, and given the book's subject, there is no reason why he

should. Still, one hopes that one day he will tell us whether he thinks Charlemagne caused the Carolingian Renaissance or the Carolingian Renaissance drew our present-day picture of Charlemagne.

PETER MUNZ
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RICHARD VAUGHAN. *Valois Burgundy*. Hamden, Conn.: Archon Books. 1975. Pp. vii, 254. \$12.50.

The author of the recently completed four-volume history of the Valois dukes of Burgundy (1363–1477) has undertaken here "to gather some of the material and all the more important arguments and conclusions of the larger work." His book is much more intelligently written and argued than Joseph Calmette's *The Golden Age of Burgundy* (New York, 1962), the work in English that most resembles it in format.

Valois Burgundy is intended for students and the general reader. Those familiar with the earlier volumes will find little new here save a discussion of the sources. After an introductory chapter explaining the political, institutional, economic, and cultural milieu of his subject, Vaughan reiterates his conclusion that Philip the Bold, the first Valois duke, consciously laid the essential territorial and institutional foundations of a separate Burgundian state while remaining in essence a French royal prince: nearly half of his total revenue in his last years came from the treasury of his nephew, the mad King Charles VI. John the Fearless is seen as the best general of the four rulers, but as an enigmatic figure who left little mark on Burgundian institutions. Philip the Good was a generally able man who eventually outlived his usefulness. In reversing the French orientation of his father and grandfather and looking instead toward the Netherlands and the Rhine, Philip instituted a change of policy that was to lead to the sudden decline of Burgundian power in the next reign. Vaughan finds that Charles the Bold deserves more credit than he has generally been accorded as an administrator, but less as a general.

The student will find here a clear and concise description of the men and institutions that governed a large and extremely diverse territory in the late Middle Ages. Valois Burgundy suffered by always remaining distinct northern and southern blocs that were separated for most of this period by the duchies of Bar and Lorraine. The first three dukes were reluctant to create central institutions that might have overcome the regional particularism of the various principalities of their composite state. Still, Vaughan regards the fall of Burgundy

as less the result of institutional failure than of the military disasters and imperial involvement of Charles the Bold. Charles' rivalry with Louis XI of France was a picturesque sidelight to the larger thrust of his policy.

The organization of *Valois Burgundy* leads to some repetition, but this is history in the grand manner. Vaughan is at his best with vivid descriptive passages dealing with the dukes and their court, administration, and military engagements. His stated goal, "to describe a political organization, not to write the history of territories, regions, areas," has surely been realized.

DAVID NICHOLAS
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JAN READ. *The Moors in Spain and Portugal*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 268. \$12.00.

An inveterate traveler in the Iberian Peninsula and an *aficionado* of things Moorish, Jan Read has here digressed from his accustomed specialties in drama and film to the field of history. The result of his labor of love is what the book jacket cautiously labels as "perhaps the first" popular survey of the Moors in Spain and Portugal for the English reader. Inclusion of one brief chapter on Portugal, together with a chapter tracing developments from 1492 to 1614, are the main structural elements distinguishing Read's treatment from that of W. Montgomery Watt (*A History of Islamic Spain* [1967]).

One does not look to a work on this level for originality of interpretation. Américo Castro's views on the pre-eminence of the individual and the common man for the evolution of Spanish history serve as a thesis of sorts for the survey. Lévi-Provençal's comprehensive studies have been judiciously mined for material in chapters one through eleven covering developments up to and including the fall of the Cordoban caliphate. The lack of any comparable secondary work for events beyond 1031 is reflected in the emphases and lacunae characterizing the last half of the book. Thus, Read's approach to Islamic culture, supplemented by Menéndez Pidal's monumental works, has guaranteed consideration of literary, philosophical, and religious facets of Moorish society to the near exclusion of economic aspects. Political analysis frequently halts at the level of details derived from dynastic-military chronicles. The Mudéjar element, imperfectly researched as yet, is understandably neglected, as are contemporary debates and publications pertaining to the crucial theme of cultural interpenetration in medieval Spain. The Almohade period remains as ill-under-

stood as ever, its establishment being noted as having been "sealed" by the victory of Alarcos in 1195 only to be decisively undermined by defeat at Las Navas de Tolosas in 1212. Valencia, as the scene of distinctive socioeconomic and artistic developments, is almost totally ignored; even the slight reference made to that region merits its inclusion in an otherwise adequate index.

It is regrettable that some disproven hypotheses and partisan interpretations are perpetuated. To cite a few examples, the theory of a causative relationship between Islamic frontier institutions and Christian military orders can and ought to be discarded in view of present research by O'Callaghan and others. To revere Islamic martyrs as genuine victims of their faith while speculating that persecuted Christians were "deliberately trailing their coats" reflects the aprioristic generalizations of an earlier generation of Arabist historians and ignorance of monographic literature such as that by Edward Colbert (*The Martyrs of Cordoba, 850-859*).

If Read has produced a volume calculated to disappoint the scholar, he has nevertheless striven rather successfully to please and meet the needs of the general reader. Colorful vignettes of significant personages, lively episodic digressions garnished with apt quotations from Arab and Hispanic chronicles and Moorish literature, occasional plates and maps, and a comparative chronological chart all make for pleasurable reading. Allusions within the text itself and a select bibliography compensate for lack of a conventional critical apparatus and point the interested reader toward standard secondary works pertaining to the subject. Those who pick up this book expecting an independent scholarly interpretation will be disappointed. Those who ask no more than an introduction to the Iberian phase of Islamic history in an entertaining and generally reliable fashion will be gratified.

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ROBERT IGNATIUS BURNS. *Medieval Colonialism: Post-crusade Exploitation of Islamic Valencia*. Princeton: Princeton University Press. 1976. Pp. xxiv, 394. \$21.50.

The present work constitutes the third volume of Robert Burns' magisterial analysis of Valencian society in the thirteenth and fourteenth centuries. *The Crusader Kingdom of Valencia* dealt with the role of the Church as a frontier governing institution. *Islam under the Crusaders* expounded the economy, class structure, law, and institutions of the conquered Muslim society. The present volume concentrates on economic life as reconstructed from

Christian tax records: that economic life whose revenues were decisive for the rise of Aragon-Catalonia as a dominant Mediterranean commercial and naval power.

Conquered Valencia was far more prosperous than either Aragon or Catalonia. The Christian kings respected the agricultural efficiency and the artisan and commercial skills of their new subjects. While imposing themselves as landlords, political and military sovereigns, they did their best to protect Valencian prosperity by retaining as far as possible the kinds of taxes and economic codes that had developed under Almohade rule.

Burns' sources are mainly Christian, rather than Muslim, the former having kept many more written records than did the latter. The reader will learn much about bakeries, baths, and butcheries, water distribution and salt supplies, transport and *hostelries*. The book is a veritable encyclopedia of trade descriptions, monetary values, tax definitions, and "state of the question" discussions of the primary sources, the regional and chronological variations, and the past scholarly interpretations of the individual topics being treated. In the copious footnotes the author is constantly at pains to explain the basis of his reasoning, the nature of his hypotheses when filling in gaps, and his estimate of the reliability of his documents.

Despite the technical nature of his discussion, Burns manages to create a coherent and pleasing narrative. The reader is abundantly aware of a complex market economy, of agriculture as "big business," and of all classes being engaged in artisan and seagoing commerce. Even small towns have public baths, the mark of widely diffused prosperity. There are almost no Mozarabs, and Jews are not distinctively important to the economy because of the high level of Mudéjar economic development. For some decades following the conquest, Mudéjar society is perhaps better off economically than in the last period of Almohade rule because tax rates are comparable and life is less violent. But the loss of independence slowly produces psychological depression and lowered economic vitality.

This is a unique work of scholarship, extracting the maximum amount of information from scattered tax records and blending with this information the political, psychological, and communal relations between the subject Muslims and the minority Christian rulers.

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PETER PARTNER. *The Lands of St. Peter: The Papal State in the Middle Ages and the Early Renaissance*. Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 471. \$17.50.

Peter Partner, a recognized authority on the Papal States in the Late Middle Ages and the Renaissance, here offers a continuous history of the patrimony of St. Peter from the beginnings until the mid-fifteenth century. A subsequent volume on the Papal States in the Renaissance has already been announced by the University of California Press. The present volume is an unabashedly old-fashioned political and military history, a type of work the author concedes is not in favor at the moment. Its detailed account of wars and treaties is scarcely interrupted even by descriptions of the institutions of government, *not to speak of the economic and social background*. Still, accepting it for what it professes to be and allowing the author to write his book, not the one I might have preferred, it is thorough, judicious, and reflects great learning. Although there is no bibliography and only rather superficial notes on the historical literature and the sources, the footnotes are replete with references to the best and latest scholarship.

The foreground of the work is occupied by local wars and politics, but Partner properly views his subject as a subtopic of the history of the universal Church. The Papal States were not just another episcopal patrimony, but a very partial realization of the Donation of Constantine. In the High and Late Middle Ages the concern of the popes for the patrimony was steadily growing, and idealists like Dante and Langland criticized the papacy for its secularity. The protesters were a minority, however, for the temporal power of the popes was generally accepted in the Middle Ages. If the popes possessed and ruled their lands, furthermore, wars naturally followed. The bloody hands of the popes, Partner believes, did not especially offend medieval society or erode the religious authority of the papacy.

Readers may wish to consider a few more of Partner's revisionist positions with a view toward modernizing their class notes. He gives a sober, nonromantic account of the dominance of the house of Theophylact, rejecting the lurid tales of Otto I's adherent, Bishop Luitprand of Cremona. (Learned historians continue to ruin the spicy anecdotes that used to enliven, if not enlighten, our classes. No doubt it is for the best.) In discussing Cola di Rienzo, he points out that the tribune, much admired by historians for his modernity, claimed a vision of the "holy" Boniface VIII, the great theocrat abominated by the same historians, whereupon Cola set out to avenge the outrage of Anagni. This aspect of the reformer's views has escaped the attention of most writers. Partner also proposes a new evaluation of the achievements of Cardinal Albornoz, who did "very little to secure the pacification of Italy, which was deemed a necessary prerequisite for the return of the Roman bishop to his own see." As a final example of

Partner's revisionism, he criticizes the interpretation in national terms of the outbreak of the Great Schism.

The book is somewhat carelessly written and edited, so that there are occasional obscurities.

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DOUKAS. *Decline and Fall of Byzantium to the Ottoman Turks*. An annotated translation of *Historia Turco-Byzantina* by HARRY J. MAGOULIAS. Detroit: Wayne State University Press. 1975. Pp. 346. \$18.50.

The last days of the Byzantine state and the fall of Constantinople are described by a number of historians and eyewitnesses, at least four of them Greek (Sphrantzes, Chalkokondyles, Kritovoulos, and Doukas). They represent different views, especially as regards the sizing up of the adversary forces and their leaders and the analysis of the causes for the demise of the *Historia Turco-Byzantina*, printed in no fewer than four editions (the first being that by I. Bullialdus in 1649) before the appearance of the fifth edition by Vasile Grecu (Bucharest, 1958), based on a manuscript newly discovered in the Bibliothèque Nationale. Grecu corrected errors in the previous editions and added a Romanian translation. H. J. Magoulias has now translated into English and annotated Grecu's edition.

In a lengthy introduction Magoulias writes of Doukas' life and the value of his work. The entire body of knowledge about Doukas, which does not include his baptismal name, comes from what he himself revealed. He lived in New Phokaia, where he was a secretary of the podesta; later he moved to Lesbos to enter the service of the Genoese Gattilusio family. He was on a mission to Adrianople in 1451 when Mehmet, the future conqueror, first entered what was then the Ottoman capital.

In his *Historia* Doukas describes the preparation of alum, which was very important in dyemaking, and the technique of covering cannon with oil-soaked felt to prevent it from shattering from the heat of discharge; he also reports on the large-scale conversions of Christians in Anatolia and the Balkans. In what Magoulias recognizes as a vivid and exciting journalistic style Doukas supplies portraits of key historical figures, data on the ethnic backgrounds of some of the Ottoman leaders, and information on various Ottoman practices and customs. Doukas praises Murad II as a virtuous and gentle person, kind to Turks and Christians alike; he condemns Mehmet the Conqueror in a one-sided characterization; and he commends Constantine XI for his courage while refusing to consider him a legitimate ruler because he was crowned not by the patriarch, but by a metropolitan in Mistra.

Doukas recognized the duplicity of the Westerners and condemned the two Hungarians for aiding the cause of the Turks, but he was a Unionist aristocrat and believed that some accommodation with the West was necessary for Byzantine survival. A friend of the Genoese and a pro-Latin, Doukas resented the anti-Unionist commoners of Constantinople. In fact, he looked upon the members of the Orthodox national party as "schismatics."

In sum, Magoulias has turned into English a very important document, which is enriched by his insights and additions.

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MODERN EUROPE

WILLIAM H. MCNEILL. *The Shape of European History*. New York: Oxford University Press. 1974. Pp. vi, 181. \$7.50.

The author is very modest in describing his work as a "little" essay. In 120 pages or so he traces the history of Europe from antiquity through the Middle Ages and on to 1973. Locating the "main drive wheel of historical change" in human interaction—whereby strangers meet and, as a result of the encounter, modify "familiar ways of behaving"—McNeill sets out to map the shape of European history. Those who have read his earlier book, *The Rise of the West*, will not be surprised at his attractive design. One of the many virtues of McNeill's work is that he writes about the West with a generous appreciation for the contributions of other civilizations. Like Lopéz, Cipolla, and others, the author is altogether mindful of the way in which Europe appropriated technology, business organization, metaphysics, and even God. His essay examines the extent and velocity of cultural and technological change. The effort should be applauded, for the author includes many fields, from science and technology to art and literature. The impulse behind this synthesis is also admirable: the first two chapters describe some of the leading features of traditional European historiography and pose some methodological questions. McNeill suggests that the "inherited shape" of European history is no longer intellectually negotiable. The scaffolding fails to support a platform of plausible synthesis. Formerly, leading historians of Europe saw the history of that continent as the progressive unfolding of liberty, and surely such a view does not inspire confidence. McNeill discusses other traditional models, contending that the basic crisis of present-day historical writing is one shared by Marxists and liberals alike who see

history "as unilinear, climaxing in a more perfect liberty." He connects this crisis with the loss of historical intellectual authority of which two symptoms are dwindling course enrollments and a scarcity of academic posts. This essay, in which he searches out patterns of behavior by generalizing from a "limited body of information" and by making intuitive leaps, is in fact his response to the historian's dilemma.

McNeill is right. The feeling that problems of historical understanding have epistemological difficulties, not to be overcome by naive manuals counseling objectivity, seems to be pervasive. Perhaps Theodor Adorno's diagnosis is apt: "Consciousness has lost the power to think the absolute and to bear the conditional." One response to the dilemma is to follow Braudel, the *Annales* school, and our own delight in technology while retreating from any consideration of the possibility of political, religious, or moral intervention by humans in history. We can view past worlds in their relationship to nature and apply reductionist psychological constructs so that the individual can do little to shape history, but is condemned to respond to such external circumstances as climate, food supply, population pressures, and so forth. Such a view is dolefully attractive if we believe that we are soon to be overtaken by the triumph of totalitarianism in these, the last days of capitalism. Despising politics, we can turn to a study of material existence, seeing our civilization, like so many others, as a helpless victim of secular trends. We are then of course powerless to intervene politically. Dubious of the values of civilization and contemptuous of high culture, we can be fashionable à la Lévi-Strauss and exalt the instinctive, primitive, and spontaneous in an elegant ballet of revenge against the constraints of modernity. McNeill encourages us to do more: one can honor the impulse toward synthesis, and the battle cry need not be "Give Us Nineteenth-Century Liberty or Give Us Braudel!"

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BARRY GORDON. *Economic Analysis before Adam Smith: Hesiod to Lessius*. New York: Barnes and Noble. 1975. Pp. xiii, 282. \$27.50.

In this scholarly study, Barry Gordon has made a concise and unique contribution by describing and analyzing early economic thought in the Western world. He introduces the ideas, personalities, and events of ancient, medieval, and Renaissance periods to illuminate the content and scope of modern economics.

Historians, particularly those seeking background for the trend of Christian economic

thought, will find the depth of research and analysis especially useful. Gordon assumes, however, that the reader has knowledge of the wars, population shifts, intellectual changes, and new economic patterns. The economic ideas of many lesser-known writers are skillfully woven into the major pattern of each period.

In the first three chapters covering the Greek period many new writers and their philosophies are presented. The emphasis is on the conflict between the expansionist city-state approach of the Sophists and the opposing Socratic philosophy. In the latter, a careful analysis is given of Aristotle's method, since it dominated later medieval thinking. The early Biblical, Christian, and Jewish influences, particularly the Mishnaic and the Christian Fathers, are skillfully blended to produce a significant combination with Roman legalism, which resulted in the canon law of the Middle Ages. A high point of this scholastic thought is epitomized in the writings of St. Thomas Aquinas.

The last chapters are devoted largely to the gradual shifts in Scholasticism brought about by changes in the economy. The Venerable Leonard Lessius represents the most effective efforts to try to adjust to the modern era.

Do not expect to find the writings of early mercantilists, natural theorists, or physiocrats in this study. The mercantilists are relegated to a revival of the nation-state building of the Sophists and the physiocrats to a reactionary form of a just and prosperous society of medieval times.

The lack of a bibliography is somewhat compensated for by the excellent footnotes and an adequate index.

FRANK E. DYKEMA
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JOHN DEE. *The Mathematical Praeface to the Elements of Geometrie of Euclid of Megara (1570)*. With an introduction by ALLEN G. DEBUS. (Primary Sources from the Scientific Revolution.) New York: Science History Publications. 1975. Pp. 33, reprint unnumbered. \$8.95.

MICHAEL SEAN MAHONEY. *The Mathematical Career of Pierre de Fermat (1601-1665)*. Princeton: Princeton University Press. 1973. Pp. xviii, 419. \$20.00.

John Dee and Pierre de Fermat are not well known to historians. Yet both these savants played important parts in the Scientific Revolution; both, in contrasting ways, were fascinating characters and are at present undergoing reassessment. The differences between the two "mathematicians"—I use quotation marks to suggest the wider scope the discipline had in the sixteenth and seventeenth centuries than it has today—were in part environ-

mental. John Dee, the extroverted English Protestant, moved in court circles and was heeded by monarchs, whereas Pierre de Fermat, the introverted French Catholic, was a district magistrate who seldom met his peers and was unknown to the establishment. What they had in common was the strong sense of the importance of mathematics per se as a framework for the rational world and as a marvelous tool. (Dee was also a geographer and navigational expert who once held the exploration rights to all North America above the fiftieth parallel! Fermat, moreover, did some fine work in physical optics.)

Dee's reputation has always been tinged by his involvement in alchemy, magic, and divination (Queen Elizabeth I's coronation date was chosen on his auguries); consequently his solid work gets overlooked. Perhaps no single piece can better highlight that work than his "fruitfull Mathematicall Praeface" to the Dee-Billingsley English translation of Euclid's *Elements* in 1570. This facsimile, with an illuminating introductory essay by Allen Debus, is admirable.

No definition of mathematical genius could exclude Fermat, who, unlike Dee, was very creative. Michael Mahoney's thorough survey of Fermat's life and achievements is unique. Most of us know that the salients in that achievement were in the areas of probability, of what might be termed the metacalculus, and of number theory. Fermat's Last Theorem, whose provenance I have long averred is suspiciously obscure and ill-documented, has baffled generations of amateur and professional mathematicians. A hobbyhorse cannot be mounted here, however, so I shall merely predict that Mahoney's Fermat will not easily be superseded as a definitive text.

N. T. GRIDGEMAN

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ELISABETH HANSOT. *Perfection and Progress: Two Modes of Utopian Thought*. Cambridge, Mass.: MIT Press. 1974. Pp. ix, 219. \$17.50.

JOHN MCKELVIE WHITWORTH. *God's Blueprints: A Sociological Study of Three Utopian Sects*. Foreword by DAVID MARTIN. Boston: Routledge and Kegan Paul. 1975. Pp. xiii, 258. \$23.00.

Perfection and Progress is an ambitious attempt to explain in their own terms the utopias of Plato, Thomas More, Johann Valentin Andreae, Edward Bellamy, H. G. Wells, and William Dean Howells. In one of several conflicting statements of her intent, the author maintains that her aim is not to survey utopian thought or to analyze all of its salient features, but rather to "describe certain changes" in the way utopias were constructed.

Despite her disclaimer, Hansot has indeed attempted to survey utopian "thought experiments," and she does engage in considerable analysis.

Her thesis is basically a gloss on a shorter work of Judith Shklar, who argues that with Rousseau a critical change in utopian thought occurred, from models for argument to programs for action. Hansot shows that the classical-to-modern shift occurred much earlier—in the seventeenth century—and deepens our understanding of the fundamental differences between the "two distinct utopian traditions." Authors shifted from using utopias as standards of judgment to writing action-minded, reformist critiques of their societies; from a concern with the nature and values of man to the institutional mechanisms and arrangements of society; from inability to act to the modern "recognition of [man's] ability to initiate social change and to use it for ends of his own devising." The contrasts are real, and the earlier dating of the change is convincing.

The thesis is persuasive despite Hansot's method—a nonhistorical, relentless series of *explanations de texte*, each author analyzed according to rigid categories and none placed in historical context. Moreover, her eccentric sample omits Campanella and all the influential French utopians. Above all, her reduction of twentieth-century utopian thought to H. G. Wells and W. D. Howells is a caricature. The inclusion of Huxley and Orwell might have forced her to confront a possible second shift in utopian thought, one that would have marred the neat symmetry of "two traditions."

Lacking in historical sense, narrow in method, and unjustified in its omissions, this book is a near failure. Joyce Hertzler's half-century-old narrative study still remains the best introduction to utopian thought.

The Whitworth book is less a failure than a cliché: another rearrangement of familiar historical materials to illustrate a behavioral theory. It often works, as in Erik Erikson's psychoanalytic interpretation of Luther. Like Hansot, Whitworth uses the respectable old device of a representative sample. But his three choices, unlike hers, are well suited to his purpose: the celibate Shakers, the less than celibate Perfectionists of Oneida, and the later but no less successful Bruderhof—the last serving as a kind of control group. A study in the sociology of religion, the bulk of the book consists of three extended historical summaries, based on printed sources, of each of the three sects. Tacked on are thirty-one pages of conclusions making some unexceptional comments on visions, leadership, social origins, sexual and family relations, and so on in the light of charisma, revitalization theory, repression-sublimation, and social control.

One welcomes the sociological analysis of com-

munitarian sects, but the book should have been written with a wider grasp of primary and secondary sources and a better melding of behavioral concepts and raw data.

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J. M. WINTER, editor. *War and Economic Development: Essays in Memory of David Joslin*. New York: Cambridge University Press. 1975. Pp. viii, 297. \$22.50.

This book undoubtedly achieves its contributors' main purpose: to honor the memory of M. M. Postan's successor in the Economic History Chair at Cambridge. David Joslin was a talented historian, a wise and helpful teacher, a valuable colleague, and above all a good man, which is well said in H. J. Habbakuk's short prefatory memoir. Joslin would be proud of his friends and pupils: their essays vary in length and originality, but some are very good indeed.

Edward Miller investigates with care and caution the incidence and effects of war taxation on English economic and constitutional development between 1294 and the 1340s, and he concludes that they were considerable. G. R. Elton describes how the English parliament during the sixteenth century became accustomed to demands for extraordinary taxation not only during and directly for war but for more general reasons of national security and prestige as well. Geoffrey Parker offers a persuasive balance sheet of the economic costs of the Dutch Revolt to the South Netherlands (that is, Belgium), the Netherlands "proper," and Spain; he concludes that the costs were very great. Peter Mathias points out how many of the problems and practices of the nineteenth-century public health movement were anticipated by the eighteenth-century army and navy medical departments.

Phyllis Deane concludes her statistical essay with the judgment that "in the last analysis . . . it would appear that the war of 1793-1815 . . . does not seem to have caused more than superficial fluctuations in the pace and content of the British industrial revolution." Simon Schama, in the most stylish of the essays, magisterially reviews the fiscal policies of the governments of the Netherlands during the eighteenth century and through the shocks of French occupation and dominance, and he argues that no lesser experiences could have sufficed to make the system of public finance adequate to the state's demands. Clive Trebilcock's powerfully original and striking essay insists that the failures of munitions procurement and supply during the first year of the Great War were all the less explicable and excusable for having been ex-

actly paralleled during the Boer War! Joe Lee's shorter chapter tells a similar tale of official failure: that of the German bureaucracy to anticipate, before the Great War, the food supply difficulties which war and blockade would bring with them. Roy and Kay MacLeod go very thoroughly into the Great War's exhilarating impact on the British manufacture of optical glass and its promotion of combined operations by government, science, and industry in that field. D. C. Coleman also applies himself to the 1914-18 government-science-industry nexus, telling the story of the manufacture of aeroplane "dope," a story differing from that of optical glass in its richly scandalous aspects and ambitions. Jose Harris does a cool job of demythologizing on Beveridge and his wartime report. The editor's contributions are a short, thoughtful essay, "The Economic and Social History of War," and a long bibliography of "the literature which relates to Western European warfare."

There is no doubt that there are gaps in this book, but, in the present state of war and society studies, it is extremely useful.

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MARCELLO MAESTRO. *Cesare Beccaria and the Origins of Penal Reform*. Foreword by NORVAL MORRIS. Philadelphia: Temple University Press. 1973. Pp. xii, 179. \$7.50.

A solid and readable biography in English of Cesare Beccaria has long been needed, and Marcello Maestro's book is especially timely. The European eighteenth century—in a somewhat wider context than that of the overly-written-about French Enlightenment—is at last beginning to speak to the twentieth-century western world. Among those with a great deal to say to us is Beccaria. His modern sounding *On Crimes and Punishments* was published in 1764 and has influenced penology ever since. The most controversial stance assumed by Beccaria in that work was his call for the abolition of the death penalty; and, then as now, he was opposed by liberals and conservatives alike. His ideas for today are as sharp and relevant as when he first wrote that "the purpose of punishments is neither to torture a man nor to undo a crime already committed . . . the object of punishment is simply to prevent the criminal from injuring anew his fellow citizens and to deter others from committing similar injuries. Therefore, those punishments and that method of inflicting them should be chosen which, in due proportion to the offence, will produce the strongest and most lasting impression on the minds of men, and inflict the least torment on the body of the criminal. . . . In order that a punishment may attain its object it is

enough if the harm of the punishment exceeds the advantage of the crime, and in this excess of harm the certainty of punishment and the loss of the possible advantage from the crime should be included; all beyond this is superfluous and consequently tyrannical." While expressing enthusiasm for the spirit of Beccaria's work, Voltaire's supposed liberalism was unable to accept the Italian's abandonment of the use of torture and the death penalty. Kant, too, attacked Beccaria for his stance on capital punishment. But Beccaria did and still does influence penal reform, and one such influence was the Pennsylvania state law of 1794 which provided that "no crime whatsoever hereafter committed, except murder of the first degree, shall be punished with death in the state of Pennsylvania." One must remember that men living at that time in most English-speaking areas, as well as others, who stole a horse or even a bolt of cloth might well find themselves at the noose-end of a rope—legally.

Maestro's book has filled a gap in English historiography on the eighteenth century. It is a slight book, unencumbered by scholarly apparatus. To the general reader this is an advantage, but to the academician it is a disappointment. Beccaria's involvement in economic, political, and even educational reform is suggestively touched upon, but there is room for fuller treatment, and one connecting in greater detail with the whole European eighteenth-century scene.

LAWRENCE F. BARMANN
St. Louis University

CLARKE GARRETT. *Respectable Folly: Millenarians and the French Revolution in France and England*. Baltimore: Johns Hopkins University Press. 1975. Pp. x, 237. \$10.00.

Millenarianism, as defined by Clark Garrett in *Respectable Folly*, describes a range of beliefs rather than a clearly enunciated ideology. His heterogeneous group all shared traditional Christian beliefs in Christ's imminent return, His earthly reign, and the final judgment (thus excluding from consideration eighteenth-century Frankists and secular utopians). These individuals alternated between the hope that the millennium would soon come, the assumption that men working in harmony with God's plan could hasten its arrival, and the fear that either the millennium would be postponed indefinitely or that it would be preceded by the apocalypse.

Regarding comparative history as more revealing than a study of one culture, the author investigates the impact of the French Revolution on the hopes and aspirations of both English and French millenarians. This investigation reveals

some interesting patterns. Although from different social strata, all the millenarians described were alienated from their environment. All were "seekers" before the Revolution, and most (perhaps responding to the rapid changes of mid to late eighteenth century) had adopted millenarian beliefs before 1789. But Garrett rejects earlier interpretations of millenarianism as a precapitalist expression of social oppression, or collective insanity, or social alienation. For him, millenarianism was a means whereby individuals who remained within Christianity could assimilate and express in traditional forms the hopes and fears created by an era of widespread social upheaval.

Garrett's characters lived on the fringes of society and exerted little contemporary influence. The author, perhaps in order to demonstrate the significance of his subject, strays beyond his self-imposed bounds to discuss the use of millenarian terminology by more influential men who no longer accepted Christian assumptions. His conclusion that men can only express their hopes and fears in the contemporary idiom diminished any significance that might be seen in this common vocabulary. Although orthodox Roman Catholics and Anglicans denied the worldly interpretation of the Book of Revelations common among millenarians, a generalized belief in the possibility of an earthly paradise was widespread during the eighteenth century. Thus Garrett's book describes a small group who expressed common sentiments in somewhat more bizarre terms than their contemporaries. He fails to show why these individuals remained Christian and why, as he insists, this study adds significantly to our understanding of eighteenth- and nineteenth-century history.

RUTH F. NECHELES
Long Island University,
Brooklyn Center

GEORGE DENNIS O'BRIEN. *Hegel on Reason and History: A Contemporary Interpretation*. Chicago: University of Chicago Press. 1975. Pp. 188. \$8.50.

A considerable number of works have recently appeared in English on Hegel. George O'Brien nevertheless sees a justification for his essay since no one has undertaken a careful textual analysis of Hegel's *Reason in History*, which he believes provides a key to Hegel's philosophy of history. The author wants to correct the interpretation, popularized by American and British analytical philosophers, of Hegel as a "metaphysical, speculative, antiempirical, a priorist" thinker (p. 2).

O'Brien wishes to demonstrate that Hegel was a historical thinker who stressed the role of individuality in history, not a philosophic monist as the modern analysts have claimed. Hegel would have

rejected the analysts' assimilation of thought with logical deduction, and therefore he should not be considered a speculative thinker. Hegel sharply distinguishes between explanation in history and explanation in nature, stressing that while nature is marked by regularity and repetition, history is characterized by innovation and takes into account the natural, man made, progressive character of historical reality.

If Hegel sees reason as the absolutely final purpose of history, he nevertheless understands the philosophic concept of history as nothing more than "actual history, properly understood" (p. 45). Hegel's doctrine of reason points to individuality rather than universality. His concept of causation, O'Brien believes, was deeply influenced by Aristotle. There is an inevitability in the progress of freedom from potentiality to actuality. But this inevitability does not exclude moral freedom. World history is thus not reducible to a predetermined schema. It is predetermined only in the sense that "as long as the underlying belief of society is that all men are truly free, then the course of history must be toward a form of social structure which adequately recognizes and expresses that belief" (p. 149). Hegel offers an understanding of history that is "at once empirical and a priori, individualized and yet leading to some achieved value" (p. 175).

This is not an entirely new interpretation. One may ask whether the very succinct, abstract presentation in *Reason in History*, edited from lecture notes after Hegel's death, can properly be understood without taking into account the body of Hegel's work that provides a more concrete expression of his view of history. O'Brien only occasionally considers passages from *Philosophy of History* and *Phenomenology of the Mind*, specifically the master-slave discussion. One may also ask whether the author has not underplayed the role of the "cunning of reason" in Hegel's philosophy of history, which subordinates the actions of individuals to the universal purpose of history. Nevertheless, the study serves as a useful and intelligent companion volume to *Reason in History* and a valuable corrective to the popular oversimplification of Hegel's philosophy of history.

GEORG G. IGGERS
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College at Buffalo

WILLIAM WEBER. *Music and the Middle Class: The Social Structure of Concert Life in London, Paris and Vienna*. New York: Holmes and Meier Publishers. 1976. Pp. 172. \$17.50.

The social history of music is a daunting field. Its ultimate goal must be to relate changes in music to

changes in society. But music is an essentially abstract art, with esoteric features. Sociologists rarely possess deep critical understanding of it, while musicologists tend to treat it as a world of its own.

William Weber disclaims any ambition to discover how "society affected the creative process itself." Instead, he has undertaken a minute examination of concert life in London, Paris, and Vienna during the critical decades of the 1830s and '40s. From a wide range of sources he has gathered an unprecedented volume of information, which confirms in fascinating detail several known phenomena of the period: the rivalry between the two elites formed by the old aristocracy and the new upper middle class; the widening gulf, cutting across class differences, between "classical" and "popular" music supporters; the growing independence, in matters of musical taste, of the lower middle class. The most surprising conclusion is that musical life in the three capitals, though differing in detail, was in outline much the same. It will no longer be possible seriously to describe Britain in this period as "das Land ohne Musik."

However, certain weaknesses illustrate the danger of writing about music without adequate understanding of musical styles. The author's sharp distinctions between segments of society may be justified, but no amount of scrutiny of newspapers and subscription lists can validate his equally sharp distinction between classical and popular music. Nowhere does he show awareness of the conflict between the aristocratic preference for older Italian music and the middle-class preference for modern German instrumental music. Yet this was still an issue in the period; it is particularly well documented for London. Weber has missed this point because he has categorized both types of music as "classical."

He can be faulted on methodological grounds also. His conclusion that the increase in musical activity "amounted to a real cultural explosion" is entirely based on a count of concerts mentioned in newspapers and journals, summarized in tables. But part of this "visible" increase is undoubtedly due to a rise in the advertising and reporting of concerts, which, of course, had its own related social causes. It would be wise to disentangle these contributing variables before talking of explosions.

NICHOLAS TEMPERLEY
University of Illinois,
Urbana-Champaign

PETER N. STEARNS. 1848: *The Revolutionary Tide in Europe*. (Revolutions in the Modern World.) New York: W. W. Norton and Company. 1974. Pp. 278. Cloth \$8.95, paper \$2.75.

At the 1975 meeting of the Society for French Historical Studies in Madison, Wisconsin, a session devoted to the historiography of the French Revolution drew an overflow audience of two hundred persons; a seminar to discuss work on 1848 in France drew six persons, of whom one was a student who thought he preferred diplomatic affairs to revolutions. No one would suggest that 1848 should have precedence over 1789 in interest or in research, but the revolutions of 1848 involved most of Europe and were not just riots. Why the neglect? Peter Stearns' book does not answer the question, but it provides the kind of review of the literature in the field that one needs as a starting point.

The fact that the Princeton University Press is still printing Priscilla Robertson's social history of the 1848 revolutions, which came out in 1948 in time for their centennial, suggests the paucity of good books covering more than one country. Stearns' book has the advantage of being, like the Langer paperback, shorter and more up to date. If the Stearns book makes no pretence of originality or research, and if the style is a bit pedestrian, it is a very satisfactory book. I particularly like the first chapters where he writes some sensible things about artisans and proletarians and middle-class liberals. His book was finished before some of the articles and monographs by young scholars who are working in the field of popular culture appeared, and one suspects that views on nineteenth-century workers will be altered substantially by the research of Joan Scott, Robert Bezucha, John Merriman, and others. My preference would be for more emphasis on the older chestnuts of liberalism and nationalism that seem to get less recognition in Stearns' book than is their due and often are ignored by the present generation of doctoral candidates. For Germany one thinks of Jacques Droz following in the footsteps of Veit Valentin—I know of no studies of the French, Italian, or Austrian revolutions of comparable scope—but one wonders who will be their successors and who, building on the work of William L. Langer, will write a major, in-depth, truly comparative study of all the revolutions of 1848. Peter Stearns cannot be faulted for doing what the editor of his series requested, but the challenge is still there for some mature historian to pick up.

JOHN BOWDITCH
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Continuing the elaboration of themes already well outlined in his *Il Socialismo prima di Marx* (1966), Gian Mario Bravo has collected in this volume a series of essays written and published separately between 1964 and 1969. Notwithstanding their self-contained character, however, the whole hangs together and provides a fairly extensive critical account of available works concerning a phase in the history of socialism that has remained relatively unexplored. Unlike the Second and Third Internationals, which have been studied in depth primarily because of their political consequences, the ambiguities of the First International have been largely ignored.

Through detailed and penetrating examinations of some of the best historical research in the field to appear during the past ten years, Bravo surveys some of the fundamental issues of the period and the most interesting figures. Thus, extensive reviews of Werner Kowalski's *Vorgeschichte und Entstehung des Bundes der Gerechten*, Edmund Silberer's *Moses Hess: Geschichte seines Lebens*, Renée Lambert's *Mouvements ouvriers et socialistes (Chronologie et bibliographie): L'Espagne*, and Arthur Lehning's three volumes of the *Archives Bakounine*, supplement the core of the book, an eighty-page bibliographical essay dealing with research carried out during the century following the foundation of the First International in 1864. For Bravo, "the First International represented in the last century a movement which, beyond the prejudiced judgments of frightened contemporaries, went beyond the very organizational schemes that it has posed . . . to assume a broader meaning: i.e., the insertion of the lowest social strata at the highest level of the class struggle" (pp. 170-71). Following Marx, he considers that phase of the history of socialism to have closed with the blood bath of the Paris Commune.

Questions concerning the relation of the various figures with the French Revolution, the connection with the birth of Zionism (in the case of Moses Hess), and the legacy that was bequeathed to the Second International have not yet been fully dealt with, and, most of all, a complete history of the International Workers' Association remains to be written. In undertaking any of these tasks, future historians will find Bravo's book an excellent bibliographical map to the literature on the subject.

PAUL PICCONE
Washington University

GIAN MARIO BRAVO. *Il socialismo: Da Moses Hess alla Prima Internazionale nella recente storiografia*. (Publicazioni dell'Istituto di Scienze Politiche dell'Università di Torino, number 23.) Turin: Edizioni Giapichelli. 1971. Pp. 285. L. 3,500.

NORMAN LEVINE. *The Tragic Deception: Marx Contra Engels*. (Twentieth Century Series.) Santa Barbara: Clio Books. 1975. Pp. xviii, 259. Cloth \$15.00, paper \$4.95.

The question that lies behind Norman Levine's *The Tragic Deception* is a poignant one: why has the anthropological vision of Marx's early communism not been realized in the goals and results of modern communist movements? The answer is that Engels misunderstood Marx's message and set his would-be followers on a wrong path. In reducing humanist Marxism to a mechanistic and unilinear theory of technological determinism Engels opened the way for the positivistic dogmatics of the Stalinists.

That Marx and Engels had different perspectives is not a new idea; yet probably no one before Levine has painted the contrast so sharply. It stemmed from "a void in Engels' intellectual equipment." Whereas Marx saw history as the developing locus of human praxis, Engels could only conceive a metaphysical subordination of men's purposeful creativity to outside powers. His mature equation of history with technological change matched his early "right Hegelian" identification of the active force as the Absolute Idea. Only Engels was a historical materialist: Marx was a "dialectical naturalist."

There is a kernel of truth in this. Marx and Engels came to communism in different ways and remained different people. Engels had little of Marx's philosophical imagination. The Paris manuscripts and the *Grundrisse*, both of which Marx kept to himself, might have surprised Engels. But the two friends did not live in different mental universes. Levine identifies Marx too closely with Feuerbachian humanism and separates Engels too strictly from it. He fails to take Marx's critique of Feuerbach seriously, or to consider the positivistic tendencies visible in his writings. The 1859 preface to *A Contribution to the Critique of Political Economy* is simply ignored, and the famous statement about windmills, steam-mills, feudal lords, and industrial capitalists from *The Poverty of Philosophy* is not even explained. The Feuerbachian elements in *The Condition of the Working Class in England* are not mentioned, and evidence which might justify the assertion that Engels "did not understand what Feuerbach meant by 'species being'" is not presented (what could it be?). The claim that Engels in 1844 "did not reject British classical economics" is simply untrue, and it hides Marx's original debt to Engels'—Feuerbachian—critique of political economy.

The level of discourse in this book is not high. Two pages separate the avowal that "Marx was attempting to surmount the Hegelian and idealist reduction of life to pure consciousness" from the (false) assertion that, for Marx, "only consciousness could have a history." One is equally puzzled to be told that "in Marx's lexicon, power had nothing to do with imperium, but dealt rather

with full development," and to learn that "Marx envisioned himself as the successor of Robespierre and Babeuf." *Hic Rhodus, hic salta* does not mean "Here is the rose, here dance," nor was that what Marx thought revolutionary conditions "cry out."

Levine asks the wrong question. Even if Engels had made of Marxism something Marx did not intend, it would not follow that he is responsible for what history has made of it. As Marx understood, the causes for such transformations lie deeper.

JERROLD SEIGEL
Princeton University

FELICIAN PRILL. *Ireland, Britain and Germany, 1871-1914: Problems of Nationalism and Religion in Nineteenth-Century Europe*. New York: Barnes and Noble. 1975. Pp. 196. \$22.50.

One would not hear today, as I heard ten years ago from an American member of the Conference on British Studies, that one could teach modern British history and completely ignore Ireland. Gladstone's remarks in a letter from Germany in 1845 are applicable for the last 130 years: "Ireland, Ireland! That cloud in the west, that coming storm, the minister of God's retribution upon cruel and inveterate and but half-atoned injustice!" Not only Irish-Americans but dissident national minorities all over the world were influenced by and interested in Irish nationalism, including the many nationalities in Austria-Hungary, the founders of the Congress party in India, the Breton nationalists in France, and members of the Flemish national revival in Belgium, to name a few. Studies of the Irish Question in this dimension could be extremely valuable.

Felician Prill, former ambassador from the Federal German Republic to the Irish Republic, has published a well-written essay on the German response to the Irish Question. It is unfortunate that his effort is at best suggestive and in general very superficial. One wonders, for example, how the Bavarian view compares and contrasts with the Prussian. There is little analysis. One is only tantalized with a few quips in editorials and the marginal scrawls of the Kaiser on diplomatic dispatches. One learns that an address from the Orange Order during the Kulturkampf was viewed as unwelcome support by Bismarck and William I. The German papers often had marvelous insights into Anglo-Irish relations, such as an editorial on Asquith's role during the Ulster crises of 1914: "The Prime Minister is such a perfect *cunctator* that if he were asked officially what time it is, he would give an evasive reply either by postponing it to the forthcoming week or by requesting a question in writing for the next ses-

sion." While it is interesting to read that Bismarck compared the Parnellites to "our Fenians" in Alsace-Lorraine and Polish Prussia, it is regrettable that the contrasting attitudes throughout Germany are not explored in depth.

JOSEPH M. HERNON, JR.
University of Massachusetts,
Amherst

GRACE P. HAYES. *World War I: A Compact History*. Introduction by R. ERNEST DUPUY. New York: Hawthorn Books. 1972. Pp. xii, 338. \$9.95.

CORRELLI BARNETT. *The Swordbearers: Supreme Command in the First World War*. Reprint. Bloomington: Indiana University Press. 1975. Pp. xv, 392.

The unabated flow of books on World War I demonstrates the spell the war continues to cast. The main area of interest remains military events, which these two studies examine in quite different ways. While claiming to offer a "vivid and dramatic account," Hayes' virtues lie more in clarity, conciseness, and coverage. Within relatively limited space, she discusses not only the familiar western front but also the Russian, Italian, and extra-European campaigns. The general metamorphosis of the war from mobility, through frustrated efforts to break the stalemate, to final battles emerges clearly. The author's approach is not analytical; she presents a narrative of established rather than innovative interpretations. Though comprehensive, the book deals briefly with particular events and individuals. Standard secondary sources are used; the facts are accurate; and the maps are helpful. In sum, Hayes provides a useful introduction to the war.

Barnett concentrates on four commanders and their major campaigns: von Moltke and the Marne, Jellicoe and Jutland, Pétain and the 1917 mutinies, and Ludendorff and the 1918 offensive. His engaging style makes men and events come alive, though sometimes he lapses into purple prose and theatrical terminology. Focusing on military events and personalities, he manages to relate them to political decisions, social structure, and technological developments. The account, based mainly on secondary sources, is historically accurate and is embellished with excellent maps and numerous photographs.

The author questions established judgments. Much history of the war has been involved with "might-have-beens"—the possibility that individual decisions or coincidental events might have altered the course of the war. Barnett generally rejects this view, arguing that the outcome of supposedly critical events was largely determined by circumstances. The war broke out as a result of "international problems insoluble in a world of

sovereign powers except by war" (p. 11). The German failure to gain a decisive victory at the Marne occurred because "the two sides were at rough parity of physical and moral forces," less because of immediate decisions than long-term developments such as German technical training and above all lack of actual war experience since 1870 (p. 96). After the Marne, neither side could win on the battlefield. The Central Powers and the Allies "were in a state of balance in terms of military and industrial power," the origins of which "lay in the long and roughly equal preparations of each side for war" (p. 97). British failure to win at Jutland was likewise attributable not so much to "accidental [events] . . . , bad luck . . . , [or] blame on individual officers . . . [as to] a vast process of dissolution that began about 1870" in British technology and society (p. 188). The German attempted breakthrough in 1918 was likewise virtually doomed from the start because of insufficient superiority (pp. 295, 297, 301). Even had it occurred, Allied leaders would probably not have surrendered since they could count on imminent American aid (p. 282). Above all, "once the war had begun, it moved according to its own inner dynamics, unrelated to policy or intent" (p. 208).

It is unfortunate that Barnett obscures this thesis by declaring in his introduction that "the theme of this book is the decisive effect of individual human character on history" as shown at moments "when the course of the war pivoted on [individual] judgement and will" (p. xv). "Vast historical consequences depended on Moltke's qualities as a man and as a commander . . . for he alone of all the sanguine generals of 1914 enjoyed a genuine chance of making it a short, old-fashioned war" (p. 19). Jutland "might have broken the deadlock" (p. xv). This tendency may stem from a desire to attract readers by heightening the dramatic, but it does so at the cost of analytical consistency. The outcome of battles was determined either by general circumstances or "individual human character," but not both.

Barnett's ambiguity is nonetheless useful in demonstrating the need for clarity. It is necessary to distinguish between the hopes of participants and the actual possibilities as evaluated by historians. Historical importance should not be confused with drama. Turning points such as great battles must be distinguished from developments which caused and perhaps determined them. Above all, in calling attention to the military, political, technological, and societal constrictions upon military commanders, Barnett encourages re-examination of the often facile condemnation of World War I strategists.

L. L. FARRAR, JR.
Boston College

JOHN WHEELER-BENNETT. *Knaves, Fools and Heroes in Europe between the Wars*. New York: St. Martin's Press. 1975. Pp. 200. \$10.00.

Sir John Wheeler-Bennett's monographs and biographies have always had a veiled element of the memoir about them. This, the last book before his death, is frankly a memoir, and an excellent one. It covers the first half of his life, concentrating on experiences in Germany, Austria, and Czechoslovakia between the wars.

The work is in part analytical, but chiefly anecdotal and personal, effectively drawing individual portraits. A moderate conservative with champagne tastes, a love of horses, and a private income, the author seems to have fancied himself the Lord Peter Wimsey of high diplomacy—a serious dilettante desiring proximity to the centers of power, expert at assessing the actions of the great and near great; someone who moved comfortably in high society and had access to Lord D'Abernon, Brüning, von Papen, Gröner, von Seeckt, Dollfuss, Jan Masaryk, Beneš, and many others. He generally drew the line, however, at Nazis, whom he despised.

Sir John records some of his surprises, such as how he found the exiled Trotsky almost friendly, yet for the first and only time he felt himself "in the presence of someone who was motivated largely by hatred . . . abstract hatred" (p. 104). In one of the book's best anecdotes the exiled Wilhelm II described how on Sundays during his boyhood, when he was with his imperial grandfather, the latter would take up a bottle of champagne, pour himself a glass, one for the boy, and in time another for himself. "Then he would carefully re-cork the bottle and mark the height of the contents on the label with a pencil" (p. 187). How the butler must have cherished the emperor's Prussian thriftiness!

Former President Beneš told Wheeler-Bennett how in the Munich crisis the Soviet minister repeatedly conveyed escalating assurances from Stalin and Litvinov that the USSR would stand by Czechoslovakia, regardless of French action. Beneš added that he gave in to the Munich diktat not because of nonsupport from his allies, but because key elements in his own state—chiefly the army and the Agrarians—would not risk securing Russian support exclusively and indeed preferred the Germans to the Russians.

All in all, this is a memoir that is always interesting, often absorbing, and occasionally startling.

EDWARD V. GULICK
Wellesley College

MICHAEL ARTHUR LEDEEN. *Universal Fascism: The Theory and Practice of the Fascist International, 1928-1936*. New York: Howard Fertig. 1972. Pp. xxi, 200. \$9.50.

NICOS POULANTZAS. *Fascism and Dictatorship: The Third International and the Problem of Fascism*. Translated from the French by JUDITH WHITE. Translation editors JENNIFER and TIMOTHY O'HAGAN. Atlantic Highlands, N.J.: Humanities Press. 1975. Pp. 366. \$16.50.

GERHARD SCHULZ. *Faschismus-Nationalsozialismus: Versionen und theoretische Kontroversen, 1922-1972*. Berlin: Propyläen. 1974. Pp. 222.

The phenomenon of fascism continues to provoke an inordinate variety of scholarly interest. It is still possible to find an author (Michael Ledeen) able to plunge enthusiastically into an examination of the "emotional element" in the doctrine of fascism. Another (Gerhard Schulz) can put together a whole book of synopses of the historiography of fascism-National Socialism. And a third (Nicos Poulantzas) has spun out webs of convoluted rhetoric in an attempt, welcome only to the already initiated, to bring the structuralist message of Lévi-Strauss and the reshaped Marx of Louis Althusser to bear upon, of all things, the Comintern's mistakes in its analysis of fascism.

Ledeen provides a well-documented study of the doctrines known as *Fascismo Universale* that were in vogue with younger Fascist intellectuals in the late 1920s and 1930s. He holds that the generation coming to maturity during the first decade of Mussolini's rule became disenchanted with the achievements of the Duce, in particular because he had failed in the mission of transforming the world through the spread of fascism on the energies of youth. This criticism, Ledeen shows by detailed study of the plethora of journals of the movement, was expressed openly, though in the required Fascist mode, by older theorists like Giuseppe Bottai and younger activists like Gastone Spinetti and Mussolini's own son Vittorio. The doctrine, however, was as mushy in its themes as most of the rhetoric of Italian fascism; and Ledeen fails, though through no fault of his own, to provide proof that there was a serious political doctrine available for export. The failure of the Congress of Montreux, organized in 1934 by the Action Committees for Roman Universality to prepare the way for foundation of a Fascist International, was only to be expected.

Ledeen provides an interesting glance at side-lights of the Mussolini regime, such as the School of Fascist Mysticism, and he gives a glimpse into the thinking of a restricted group of younger Fascist leaders. But it would have been far more valuable if he had provided a genuine study of a generation, an analysis of the cohort with which he seeks to deal that would go far beyond quotations from obscure journals.

Schulz has impeccable qualifications for producing a historiographical synthesis of writing on fas-

cism and National Socialism. He is Professor of Contemporary History at the University of Tübingen and author of several well-received books on the Weimar and Nazi periods. The present book, however, is something of a disappointment. He admits that this compilation of summaries of interpretations is a *Nebenarbeit*, the necessary accompaniment to the writing of a bigger book. One cannot avoid the impression that the author felt that since he had to read through all these works anyway, he might as well put his notes together as a book.

Schulz makes insufficient effort to analyze the historiographical trends within the fifty years he is considering. No pattern of synthesis emerges in his treatment, in part because he wishes on principle to place his authors in their historical context. Had he chosen to organize his materials around themes like those suggested by Karl Bracher in the opening chapter of *The German Dictatorship* (1972), we would have had a more useful study. Instead, Schulz limits himself to giving, one by one, a brief exegesis of the opinions expressed by almost two hundred authors. But his treatment is very uneven. His primary interest is in the 1930s. As a result, the early writers on fascism, such as Luigi Salvatorelli, receive only brief mention, while the vast mass of writing since 1945 receives even shorter shrift. By contrast there is extended treatment of such authors as the Flemish socialist-turned-nationalist Hendrik De Man or the secessionist German socialist Fritz Sternberg. The author unfortunately makes little effort to place these figures in a historical context, either by nation, by ideology, by methodology, or by intervening historical event. The book is thus a cursory introduction to a number of neglected writers of the interwar years. It is not the needed historiography promised by its title.

If occasionally one wishes Schulz would give us more, with Poulantzas one frequently wishes he would stop. This is a book for the inner circle, the disciples of that little group of political philosophers who gathered around Louis Althusser at the École Normale Supérieure in the 1960s. At that time Althusser, a distinguished teacher in that hothouse of the French intellect and a member of the Central Committee of the Communist party, was arguing that the lessons of the structuralist movement in linguistics and anthropology could be applied to a fresh study of Marx. In two intricate studies now published in English as *For Marx* (1969) and *Reading Capital* (1970), he proclaimed the uncompromisingly dogmatic Marx, a structuralist before his time, as George Lichtheim has shown in a delightful essay in *From Marx to Hegel* (1971). Poulantzas established his credentials with the Althusser group in 1968 with *Political Power and Social Classes* (English translation, 1973), an enormously involved study of the capitalist

state. For the historian, *Fascism and Dictatorship* is a more interesting book. The author's method is to pose a series of theses and then to take concrete examples from Germany and Italy as proof of the correctness of his views. For example, in his chapter "Fascism and the Class Struggle," he posits that fascism is a specific stage in the rule of the dominant classes. Finance capital or big monopoly capital enters into conflicts with the other members of the dominant classes. It attempts to assert its pre-eminence in the political sphere. There is a breaking of ties between the existing political parties and the newly assertive forces of finance capital. A crisis occurs in the dominant ideology, which further strains the relationship between the political parties and the makers of ideology. The working classes are defeated in a trial of strength, and the dominant classes in turn go on the offensive. The Fascist party comes to power because it can mediate the contradictions within the dominant classes and between the dominant classes and the dominated classes.

Throughout the book this form of analysis is accompanied by a study of the mistakes of the Comintern, on the grounds that an understanding of its policy and principles not only illumines the nature of fascism but also explains the situation of the labor movement in Europe that still bears the stamp of the Comintern. The later chapters contain detailed studies of the dominant classes, the working class, the petty bourgeoisie, and the inhabitants of the countryside.

At the end, when the constant repetition, the wearisome asides, and the Marxist jargon are removed by the reader, the historian finds few ideas that are suggestive of new ways of interpretation. He may, however, come away with a greater understanding of the Comintern. He will definitely better comprehend what is fashionable today among one section of the Paris intelligentsia.

F. ROY WILLIS
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Davis

CHARLES S. MAIER. *Recasting Bourgeois Europe: Stabilization in France, Germany, and Italy in the Decade after World War I*. Princeton: Princeton University Press. 1975. Pp. xiv, 650. Cloth \$27.50, paper \$11.50.

MARTIN KOLINSKY. *Continuity and Change in European Society: Germany, France and Italy since 1870*. New York: St. Martin's Press. 1974. Pp. 234. \$16.95.

Charles S. Maier has written a long and remarkable book in which he asks the reader to rethink the years 1919-29 and to accept his cogently argued thesis that "rescuing bourgeois Europe meant recasting bourgeois Europe." The First

World War and its aftermath, he maintains, led to less social and economic leveling in Western Europe than might have been expected; prewar political and economic elites staved off all threats and were restored to power, albeit in a changed way. It is this change, the "recasting" of bourgeois Europe, that he examines in this exhaustive individual and comparative study of the three West European countries.

The fulcrum of decision-making shifted, he seeks to demonstrate, from parliamentary political parties to organized interest groups representing the industrial, commercial, labor, and agricultural classes, all more concerned with economic self-interest than with political issues and willing to desert older political allegiances; the line between economics and politics became blurred, and everywhere there was an interpenetration of state and economy. To describe the phenomenon, despite some understandable misgivings of the term for the Italian experience, he uses the word corporatism. In development of his thesis he analyzes closely—this is no narrative history—internal and external sociopolitical conflicts over such issues as nationalization, taxation, public finances, reparations, and tariffs. No one will close the book without a deep respect for Maier's mastery of detail, his ease in handling the comparative history of the three separate countries, his impressive familiarity with the monographic and archival literature, and his arresting conclusions. The equilibrium he describes, and the concomitant preservation of the European class structure, would be important but less striking if it were not the case that the arrangements of the 1920s, interrupted though they were by the Depression, the emergence of the Third Reich, and the Second World War, foreshadowed the political and economic settlements of post-1945 Europe, in a sense ushering in a "transformation of capitalism" that has lasted for over half a century. It is no small accomplishment to have reassessed in this thorough and original way the generally unsung conservative achievement of the 1920s.

In the second volume under review a political scientist at the University of Birmingham assesses "continuity and change" in European society by examining the three major West European countries in the years from 1870 to the present. With the disastrous experience of fascism in Italy, Nazism in Germany, and the Vichy regime in France in mind, he asks whether democratically constituted political systems can hope to operate successfully in environments where nondemocratic traditions are strong. For developments since 1945, the most rewarding part of the book, the author's conclusions are sobering; he sees, with variations for each country, a failure to sustain a democratic center, the division and alienation of the labor

movement, the blocking and postponement of reforms, and the continuity of economic and administrative elites from the past.

Kolinsky's study as a whole reveals no startling new insights or conclusions, but represents a fairly conventional, though sensible, summary of major European developments written with a sympathy for labor and liberal aspirations. No new ways emerge of viewing the last hundred years, nor is there any effort to reshape conventional periodization. The exclusion of the German Democratic Republic, though justified on the ground that the book deals with "self-determining" societies, limits the usefulness of the study. The professional historian will be unhappy with the sketchy documentation, the poor editing, the spotty bibliography, and the ignoring throughout of a score of key scholarly writings on France, Italy, Germany, and European integration published in the past ten years.

JOEL COLTON
Rockefeller Foundation
and Duke University

ROLAND N. STROMBERG. *After Everything: Western Intellectual History since 1945*. New York: St. Martin's Press. 1975. Pp. 274. Cloth \$12.95, paper \$4.95.

In many ways this is an impressive book. In nine short chapters Roland Stromberg has undertaken an amazingly comprehensive survey of major trends in recent philosophy, literature, art, music, the social sciences, political attitudes, and social patterns on a broadly Western scale.

Yet this is more than a useful supplement to Stromberg's *Intellectual History of Modern Europe*. It is a critical examination of the quality and direction of civilization in a postmodern world. Stromberg's thesis that "the whole pervasive life-world of urbanized, democratic, technologized man germinates the doctrine of incessant mobility" (p. 90) and the search for "incessant novelty" (p. 117) is not new. He carries this thesis to the extreme conclusion that all common values of society are in the process of collapse, that intellectual creativity has run its course, and that the art of modern times, as well as literature and philosophy, have "become absurd, meaningless, rooted in nothing except the stray and puerile insights of rootless individuals" (p. 94). "Mere anarchy is loosed upon the world" as the new "barbarians rise from the catacombs" (p. 235).

The pervasiveness of this thesis leads to a certain one-sidedness in presentation. Stromberg is a political conservative, who however realizes that no return to the past is possible. He maintains a special ire for welfare economics, Marxism, and the New Left. He fails to consider the economic

and social problems of modern industrial societies that have led to these responses. Marxism is treated as an economic superstition of the nineteenth century that has little relevance to the analysis of the modern social order. The importance of the New Left is very much overstressed; the political aims of the protests, such as occasioned by the Vietnam War, are virtually overlooked; and members of the student movement are portrayed as "barbarians from within," "potential destroyers of a weakening civilization" (pp. 141-43). Even the mild concern of secular humanists or Christian theologians with problems of social reform is berated as a dangerous betrayal of their intellectual and religious integrity.

This tendency to oversimplify the causes and manifestations of modern malaise detracts from the seriousness of a basically informative, stimulating book that deserves to be read and discussed.

GEORG G. IGGERS
State University of New York,
Buffalo

GEORG G. IGGERS. *New Directions in European Historiography*. With a contribution by NORMAN BAKER. Middletown, Conn.: Wesleyan University Press. 1975. Pp. ix, 229. \$16.00.

This new book confirms Georg Iggers' place as the foremost guide to the historiography of Continental modern Europe. He established this position in his earlier volume, *The German Conception of History*, in the Ranke anthology he co-edited and introduced with Konrad von Moltke entitled *The Theory and Practice of History*, and in assorted articles, of which the most general is the essay on historicism in the *Dictionary of the History of Ideas*. None of these earlier studies ignored recent historiography, but they focused on what might be called the classical period of modern historical literature—that is, the "scientific" history of the nineteenth century and its modulation around the turn of this century—with more current tendencies serving as its diffracted conclusion. Now the author has reversed the emphasis. In the book under review he does not neglect the nineteenth- and early twentieth-century background, but this context is now prolegomenous to Iggers' main concern with the striking and provocative innovations in historical thinking and historiographical practice that have marked our own generation.

It is important to understand what Iggers means to include and exclude, for his book is not intended to survey the entire spectrum of recent historiography. Consonant with his stress on the autonomous "scientific" strand of nineteenth-century historiography and its end-of-the-century crisis, Iggers' "new directions" are selections from the

three most prominent European efforts to retrieve the scientific claim of history—the structural history of collectivities associated with the *Annales*, the social history of politics, and the professional sophistication of Marxism. In Iggers' laudably qualified approach to historiography he sees it not as an insulated set of technical methods but rather as an expression of the cultural era to which it belongs, and by dint of this contemporary influence, he tends to identify scientific history with social history (taken in the intermediately broad sense to include economic history). The author's persistent references to the institutional conditioning of historical scholarship lead him to define historiographical tendencies in national terms: structuralist history is French; the social history of politics is West German; even Marxism, superficially transnational, is suborganized into separate East German, East European national, and French sections, with the British variant segregated in a separate contribution by Norman Baker.

Within these self-imposed limits, Iggers' analysis exhibits all the virtues we have come to expect. It is thorough in its coverage, apt in its choices, penetrating in its perceptions, balanced in its judgments, and thematic in its construction. The theme accommodates the blend of sympathy and criticism that is the hallmark of the book, and it provides a substantive unity to the parts over and above the formal coherence supplied by the emphasis on scientific desiderata and the social dimensions of history. Iggers' theme is the historiographical necessity of joining general truths, delivered by the methods and concepts of the new social history, to the individual life of men, events, and ideas, which has long constituted the truth of traditional history. Not only does this theme work in that it makes an authentic book out of four component essays, but, in my opinion, it is precisely right: the juncture of generality and individuality remains the crucial task of modern historiography.

Because the "European" in Iggers' work patently refers to historians rather than to history, we of American vintage are not immediately, but indirectly, affected by it. Those of us who are not participating in the kind of historiographical movements that Iggers covers will perhaps learn more from him than those who are. But his focus also leaves those not engaged in the new scientific social history with a final ambivalence, with a feeling of unease at being out of the swim, and with a sense of pride at our nonconformity.

LEONARD KRIEGER
University of Chicago

J. R. RAVENSDALE. *Liable to Floods: Village Landscape on the Edge of the Fens, AD 450-1850*. New York:

Cambridge University Press. 1974. Pp. xi, 206. \$15.50.

Thanks to the earlier pioneering work of Professors Hoskins, Finberg, and Beresford, much is now known about the general history of the English landscape and the marks that man has made upon it. In more recent times the general survey has given way to the regional study, often that of a county, sometimes that of a district with a particularly interesting tale to tell. Thus M. Williams' interesting monograph showed how the marshes known as the Somerset Levels were drained and became a highly productive agricultural area. Now J. R. Ravensdale has published for comparison a very valuable study of changes in Cambridgeshire.

Liable to Floods is a detailed topographical and social history of village landscapes on the edge of the Fens from 450 to 1850. The villages are Landbeach, Waterbeach, and Cottenham, whose very names stimulate the reader's interest. Wisely taking a long period, Ravensdale analyzes these three communities' management of their resources: water, fen, meadow, pasture, and arable. He demonstrates the changing importance of such occupations as fishing and fowling, as well as the eagerness of the fenmen to defend their customary rights. He shows how the communities responded to changes in both population and the water level, and he analyzes the field patterns and the topography of the villages themselves.

This is, then, a very interesting book that includes much detail, not all of it well digested or elegantly composed, though it remains true that in works of this kind it is detail that matters. Although the author has tested a number of accepted generalizations about the Fens, it is a pity he has seen few opportunities for comparison with other localities. Nevertheless, his conclusions are both carefully drawn and satisfying. The value of this scholarly monograph is enhanced by its thorough documentation and its clear, informative maps.

GORDON C. F. FORSTER
University of Leeds

FRANCIS PIERREPONT BARNARD, editor. *Edward IV's French Expedition of 1475: The Leaders and Their Badges Being MS. 2. M. 16. College of Arms*. Reprint. Gloucestershire: Gloucester Reprints; distrib. by Rowman and Littlefield, Totowa, N.J. 1975. Pp. xv, 162. \$17.50.

Edward IV's expedition to France in 1475 was the largest any English king had ever led to the Continent, but it was inconsequential for it returned home without striking a blow after Charles the Rash of Burgundy deserted his English ally. The book under review, a reprint of a 1925 publication,

is not an account of that expedition but a listing of the leaders, the number of their men, their pay, and their badges.

The book is based on an eight-page manuscript belonging to the College of Arms, which is both reproduced in facsimile and transcribed. The remainder of the volume consists of biographical sketches of the sort found in the *Dictionary of National Biography*. "Biographical" is perhaps the wrong word, although it is used on the dust jacket (substituting for the author's "personal," which is not better), for the short pieces consist of parentage and provenance, wives' names and origins, titles and offices held, battles at which they were present, and other dry bones, useful for reference, but enlivened only very occasionally with a tiny bit of flesh and blood. The last few entries include clergy, kings of arms, ordnance specialists, and engineers. An earlier document provides the names of physicians and the king's secretary.

It is the armorial badges that really interest the author, although not this particular reader. Almost all the entries in the manuscript contain a description of the leader's badge and a crude drawing of it. Along with each biographical sketch is another entry describing the leader's badge (a black bull, a silver scallop, a blacked ragged staff, and so forth) with its derivation (a pun on the surname or place name, from the wife's family, and so on) and other armorial information.

Both biographical sketches and essays on the badges are done with a great deal of erudition, which many historians of the new school will no doubt feel might have been expended toward a more significant end. Even they may find the work of some use, however, because it gives the organization and pay of a very large, late medieval army.

DONALD E. QUELLER
University of Illinois,
Urbana-Champaign

ALISON HANHAM. *Richard III and His Early Historians, 1483-1535*. New York: Clarendon Press, Oxford University Press. 1975. Pp. vi, 230. \$23.00.

Fans of Josephine Tey's *Daughter of Time* and Fellows of the White Boar notwithstanding, Richard III should now be placed back into history's rogues gallery. With far more detailed detective work than Tey's imaginary inspector, Hanham presents a critical portrait of Richard based on accounts, which could not be distorted by Tudor propaganda, written before Bosworth. Horace Walpole's essay of 1768 (*Historic Doubts on the Life and Reign of King Richard the Third*) accused Henry VII of employing his reign "not only in extirpating the house of York, but in forging the most atro-

cious calumnies to blacken their memories and invalidate their just claim." Now that Walpole's hypothesis has been disproved, his criticism of blind, dogmatic historians can be applied to the "fanatics of his own party." The obscurity of Richard's reign is mainly the result of the paucity of fifteenth-century records. It is doubtful whether "consistent distortion for political and dynastic ends" can be charged against any of the historian-chroniclers Hanham examines. The serious distortion occurs in Shakespeare's *Richard III* where, for example, the playwright turns crooked shoulders into a hunched back.

Hanham's book is mainly a series of historiographical essays on the historians and chroniclers who wrote about Richard III between the time of his usurpation in 1483 and the death of Thomas More in 1535. Following several of the essays is an excursus or kind of biblical exegesis of the documentation. The most fascinating chapter, "Sir Thomas More's Satirical Drama," analyzes the philosopher-saint's *History of King Richard the Third*, which many literary critics and historians consider factual because of "the superior illusion of reality that is the product of art." More's use of parody can be subtle and tricky, especially if taken at face value.

Hanham's study requires careful reading and a good knowledge of the period. With its skillful probing and speculation, however, it is a model historiographical essay.

JOSEPH M. HERNON, JR.
University of Massachusetts,
Amherst

HENRY ANSGAR KELLY. *The Matrimonial Trials of Henry VIII*. Stanford: Stanford University Press. 1976. Pp. xii, 333. \$15.00.

Henry Kelly's study concentrates on neglected procedural and canonical aspects of the matrimonial trials of Henry VIII. The largest portion of the book examines the king's efforts to obtain a divorce from Queen Catherine of Aragon, but it also includes accounts of litigation leading to the dissolution of his marriages to Anne Boleyn and Anne of Cleves. Throughout, the author concerns himself with issues of canon law rather than with the conflict of personalities that makes the king's "Great Matter" one of the great dramas of the sixteenth century. Kelly contends that the trials have never been properly understood because the implications of Henry's "relationships with Catherine and Anne have been improperly evaluated" and "key documents detailing his program of justification have been ignored or forgotten." He argues specifically that the records of the three trials convened in England to pass judgment on the

king's marriage to Queen Catherine have been neglected. According to Kelly, J. S. Brewer, the nineteenth-century editor of *Letters and Papers of Henry VIII*, was the last person to study the official record of the trial initiated by Wolsey in 1527. Twentieth-century historians have likewise ignored manuscript records of the trial convened by Wolsey and Campeggio in 1529 and Cranmer's trial in 1533.

The author, in re-examining the king's much-studied conscience, finds evidence in Cranmer's *Articuli Duodecim* that Henry was deeply troubled by memories of his wedding night when he found Catherine a virgin. The resourceful Cranmer salvaged the king's conscience with the argument that her previous marriage to Prince Arthur could have been consummated with carnal copula even without the loss of her irrecoverable virginity.

Kelly reveals the complexity and ambiguity of late medieval canon law affecting marriage, and to at least one reader makes Henry's vexation with the Church more understandable.

BARRETT L. BEER
Kent State University

G. D. RAMSAY. *The City of London in International Politics at the Accession of Elizabeth Tudor*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. viii, 310. \$21.50.

Many historians of Tudor England have long been concerned with the problem of the national government's finances and the Tudor price revolution and inflation. We now have a detailed study of how the government of Elizabeth raised funds and made fiscal policy prior to the rise of Sir Thomas Gresham as the Queen's royal agent. This work, however, is more than just a financial account, and it is more than an account of royal finances. G. D. Ramsay deals with the city of London and its merchants and with private as well as public funding that established the base for the extensive growth of both London's and England's foreign trade.

This work is part of a projected study on the ending of the Antwerp Mart as the center of international finance, and it fills a major gap in our knowledge of the period. Starting with an analysis of Antwerp at its height and with London as a "satellite" city, the work deals with the political and diplomatic background of the attempts of London and its merchants to establish a national domestic money market. It then treats the larger aspect of Anglo-Netherlands politics and trade. Attention is given to various attempts to establish alternate financial centers both to the north along the Baltic and to the market at Emden—all attempts to cut into Antwerp's control.

Ramsay's study is an excellent start and, we hope, will be followed by the much-needed and long-overdue studies of Gresham, the shifting of international financing that began in the late 1560s, and the gradual establishment of London as an important money center. We have in this volume a well-documented study of the initial phase of this change. It is regrettable that the price may discourage some from obtaining this study, but graduate students and others interested in this problem will not err in starting with this book to learn about early Elizabethan international finance.

M. C. ROSENFELD
Southeast Massachusetts University

S. J. WATTS with SUSAN J. WATTS. *From Border to Middle Shire: Northumberland, 1586-1625*. Leicester: Leicester University Press; distrib. by Humanities Press, Atlantic Highlands, N.J. 1975. Pp. 288. \$21.75.

Despite being "altered and rewritten," and although improved by studies on geography, agrarian economy, and enclosures by the author's wife, this book has not progressed beyond the useful dissertation that preceded it.

All students of the local history of Tudor and Stuart England will welcome the facts on border institutions, the description of land tenures and regions (and how these affected peace and productivity there), and the identification of eighty-nine heads of the leading gentry families. This covers about half the book and constitutes "the economic and social situation" in 1603. But readers will look in vain for any detailed survey of the actual lands—their locations and tenurial conditions—belonging to particular gentlemen, or for any hint that landed wealth had something to do with local factions, marriages, offices, and ancestors.

The authors show the connection between royal favor and landed wealth, but they do not analyze sufficiently this connection so that one can determine how eighty-nine families became the leading ones. Moreover, they give no indication of the extent of their evidence, without which their conclusions must appear impressionistic. And what of the aristocracy and the merchants? The former are dubbed inconsequential because nonresident, while the latter soiled themselves in the coal trade. But had the Percys no local patronage after the late 1580s? Did none of the twenty-three armigerous Newcastle burghers have landed ambitions or business dealings with the gentry? It is good to have the demonic myths about Northumberland Catholics laid to rest, as the authors do, but how well did Catholics integrate into the social, economic, and political hierarchy of the county? Such questions have not been adequately explored.

In addition, unless the personal basis of wealth, local power, and rivalries are set out more systematically, one cannot make sense of the "complex and often devious political history of Northumberland from 1586 to 1625," which is the subject of the second half of the book. What should have been a decisive integration peters out in following the shuffling of officers on the commissions of the peace, tiresome border disputes, and the machinations of the Privy Council in far away London.

FRANK F. FOSTER
University of Colorado,
Denver

DEREK HIRST. *The Representative of the People? Voters and Voting in England under the Early Stuarts*. New York: Cambridge University Press. 1975. Pp. xii, 306. \$21.00.

Derek Hirst has written the first book-length study of "voters and voting" in early Stuart England. Divided into three parts, "The Electorate," "Elections," and "After the Election," and with eight appendixes, his book identifies the 1620s as "the decisive period" in the development of the electoral process. The research is impressive and the bibliography useful. Hirst has discovered pertinent material, including little-known corporation records and voting lists, in all major national archives in England and in local archives in each county.

Hirst's conclusions are avowedly "contentious." Contemporaries were confused and uncertain about the composition of the electorate and the electoral process. The size of the electorate was much larger than scholars have thought, hypothetically "some 300,000" persons by 1640. Inflation had the effect of enlarging the number of 40s freeholders who traditionally had voted in the counties. In over thirty boroughs the franchise was also broadened as a result of disputes that involved essentially local oligarchic rivalries and gentry ambitions focused on winning a parliamentary seat rather than a commitment to enlarging the franchise. The House of Commons' decisions to widen the franchise in the 1620s, promoted by men of diverse partisan interests, also manifested self-interested political considerations. Elections were freer than has been acknowledged, and the number of disputed elections increased markedly. Moreover, the electorate was increasingly aware of national issues, and the relationship between the House of Commons and the voters was much closer than has been thought. Hirst believes that by the time of the Civil War there was "some justification" for Parliament to regard itself as the "representative of the people." Such conclusions amend understanding of aspects of local and national politics and of the nature of the Civil War.

Some of Hirst's conclusions must be regarded as tentative. As the author himself repeatedly notes, the data for such topics as voting behavior are fragmentary and distorted. The most successful sections are those that deal with the social composition and expansion of the electorate, the nature of the franchise disputes in the boroughs, and the relationship between Parliament and the electorate. Less successful are the pages on the elections in 1640. Hirst dismisses in a paragraph the question of whether women voted; he omits by design the subject of patronage; and he does not always present effectively quantitative data. Furthermore, Hirst writes in graceless prose, replete with errors in grammar and syntax. As a result, many sentences are so opaque as to be impenetrable. Despite these weaknesses, the importance of the topic, the wealth of data assembled, and the potential significance of the conclusions make this book valuable to seventeenth-century scholars.

LOIS G. SCHWOERER
George Washington University

CHARLES P. KORR. *Cromwell and the New Model Foreign Policy: England's Policy toward France, 1649-1658*. Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 268. \$12.50.

Charles P. Korr examines Cromwell's foreign policy from the viewpoint of relations with France during the Protectorate. The first six chapters discuss events between 1649 and 1654 for background; chapters 7 through 12 explore episodes in those relations; chapters 13 through 15 touch on the high points of Cromwell's diplomacy (the Treaty of Westminster [1653], the mission of Sir William Lockhard, and the occupation of Dunkirk after the battle of Dunes [1658]). Chapter 16 gives a retrospective view of the whole. Korr finds Cromwell motivated by strong religious persuasions, by a Puritan hatred for Spain, and above all by a need to maintain a stable, secure regime in England. To get what he wanted in foreign relations, Cromwell put off decisions, to the point that France and Spain, worried about the Protector's army and navy, eagerly competed with each other in making concessions to his demands.

Korr's study is not a work of major importance. It has no new discoveries—none being likely anyway, after the comprehensive work of Gardiner and Firth seven decades ago—and Korr's scholarly diggings, as revealed in thirty-nine pages of notes and nine pages of bibliography, seem to reinforce interpretations made by Christopher Hill, Michael Roberts, and others. When Korr has a strong opinion of his own, as he does in chapter 7, notes 17 and 19, he asserts his judgment without giving an indication of how he got there.

Two problems undermine the value of the monograph. The first is methodological. The study is "to show how Cromwell's perceptions of events caused him to construct a foreign policy that represented the priorities he had established for his rule in England" (pp. 2-3). The narrative is not congruous with that purpose. Rather than getting inside Cromwell's process for establishing priorities, Korr gets inside John Thurloe's collection of state papers. The result is a mire of diplomatic opinions, suspicions, and unverified information, the consideration of which shoves Cromwell out of the picture much of the time. The second problem is technical. The notes reveal flaws in typography and inconsistencies in form. More important, over half of the citations of documents in volume 2 of *A Collection of State Papers of John Thurloe* have deficiencies ranging from minor misquotations to the major problem of no correspondence between citation and document.

The study does not satisfy the expectation explicit in its title. A modern study of Cromwell's foreign policy remains to be written.

W. KENT HACKMANN
University of Idaho

ROYCE MACGILLIVRAY. *Restoration Historians and the English Civil War*. (International Archives of the History of Ideas, 74.) The Hague: Martinus Nijhoff. 1974. Pp. xi, 264. 70 gls.

Macgillivray is not concerned with memoirs, diaries, or autobiographies. He is concerned with seventeenth-century historians of the English Civil War and what they had to say about its origins and aims. These historians did not intend to be dispassionate; in their view, history was a polemic art. They meant to explain as well as recount what had happened. Their work was not easy. Sources were limited and partial—newsheets, letters, personal recollections, and, in many cases, each other. Official censorship, personal interests, and the outcome of events marked their attitudes. Their histories are often incomplete and uneven. Taken collectively, however, they had much to say.

Macgillivray is clearly more concerned with questions of causes and aims than were historians of the seventeenth century. His questions are pointed, their answers scattered and diffuse. Yet Macgillivray does not falsify. If the historians tended to see the hand of God in events, they did not fail to also see the shortcomings of kings, the design of conspiracy, and the importance of social and economic aspiration. Macgillivray finds some of them aware on various points. What part, for example, had Archbishop Laud's ambition played in the developing conflict, what part the in-

decisiveness of Charles I? Was the revolution a plot? Who plotted and to what end? Was religion a major issue, or did it merely mask others? Did the acquisition of monastic lands set the gentry against the king, or ally them with him? Was England too rich or too poor?

The author deals with the work of historians before the Restoration (special attention is given to Peter Heylin and Thomas Fuller), then with historians writing between 1660 and 1702, first those of royalist persuasion (Thomas Hobbes and John Hacket), then parliamentarians and Whigs (Edmund Ludlow, Lucy Hutchinson, and Gilbert Burnet). Separate chapters deal with Rushworth, Nalson, Whitelocke, Baxter, and Clarendon. Each writer is considered individually—the various printed and manuscript versions of his history, the sources and experience upon which he drew, his originality (or lack of it), and his particular interests and emphases. The historians' work is aptly characterized and the special questions they raised are explored. Why, for example, did Hobbes choose to call his history *Behemoth*? What use did Whitelocke make of the manuscripts available to him? Why did Clarendon so prize the virtues of friendship?

Useful footnotes and an excellent bibliography enhance the study.

CAROLYN A. EDIE
University of Illinois,
Chicago Circle

COLONEL BENJAMIN CHURCH. *Diary of King Philip's War, 1675-76*. Edited with an introduction by ALAN and MARY SIMPSON. (A Tercentenary Edition.) Chester, Conn.: The Pequot Press, for the Little Compton Historical Society. 1975. Pp. xxi, 226. \$12.00.

A new edition of Benjamin Church's account of his campaigns during King Philip's War is most welcome. This present volume, issued to commemorate the tercentenary of the conflict, comprises only the first part of the Church memoirs, dealing with the years 1675-76, and eliminates the second section concerning the "Eastern Expeditions" in Maine during King William's War and Queen Anne's War. It is a beautiful production, with modernized spelling, punctuation, and paragraphing, fifty-seven illustrations, notes, bibliography, index, and a long and perceptive introduction by Alan and Mary Simpson.

The book is valuable not only for the accuracy of its history but for its ethnological detail and its presentation of racial attitudes. Church liked and got along well with Indians. He was always fair in his dealings with them and inspired such admiration as a military leader that many of his Indian

captives were soon eager to switch their allegiance and fight under such a brave and resourceful war captain against their former ally, Philip. Despite this friendship, Church never understood Indian grievances or the complex causes of the war. He condemned simplistically Philip's warriors as "bloody wretches" who "thirsted after the blood of their English neighbors, who had never injured them, but had always abounded in their kindness to them" (p. 72).

The Simpsons have recognized the root cause of the conflict in the "collision of cultures which set the pattern for almost all future relations between white men and red men in English America" (p. 1). At their best, the English were paternalistic and condescending to the Indians; at their worst, they were arrogant imperialists, insisting that Indian nations be subject to them, conform to their ways, and abide by their laws. The war begun by Philip was a last desperate attempt at maintaining Indian sovereignty.

Indicative of extreme carelessness in proof-reading is the fact that not one of the cross-reference footnotes is accurate. The temporary page references in the manuscript and galleys were not altered once the page proofs were completed, and we are treated accordingly to twelve footnotes that refer to the wrong page numbers or to no pages at all.

BARBARA GRAYMONT
Nyack College

NEIL MCKENDRICK, editor. *Historical Perspectives: Studies in English Thought and Society in Honour of J. H. Plumb*. New York: International Publications Service. 1974. Pp. x, 319. \$16.00.

The names of two historians dominate the study of eighteenth-century England. Sir Lewis Namier profoundly changed ways of looking at the century and its politics and has affected the study of politics in many other times and places as well, always stimulatingly, not always rightly. His own researches were intensive but narrow; his assumptions about the nature of politics and of history were similarly narrow and sometimes malign. He has had disciples, and his methods and interpretations still bear fruit; but—to repeat a commonplace in the profession—much of that production has come since his death, perhaps because he was so devastating a critic.

The fact that Namier was and remained (or was kept) an outsider has often been remarked. J. H. Plumb, too, is an outsider: on that the personal tribute which prefaces this volume is quite candid. Yet how different has been the pattern of Plumb's writing and influence. The range of his work is

enormous, indeed prodigious—and nearly all of it, as the valuable select bibliography makes plain, has appeared since 1950. And if Plumb has not always spoken with equal authority on all the times and subjects that have attracted him, he has never failed in enthusiasm or to be stimulating, even provocative. Again, the editor's tribute shows how these qualities informed his teaching, as lecturer, tutor, and guide to research students on both sides of the Atlantic. For all the wide influence that can rightly be ascribed to it, Namier and his method have given rise to far less than we might have expected in studies of his own century. Plumb, too, has generated fewer eighteenth-century studies among his students and friends than we might like to have. Fear of the master is not at fault, however; rather the very breadth of Plumb's interests and the kind of encouragement he has given have led to historical exploration in all directions.

The ten essays in *Historical Perspectives*, all by former pupils of Plumb, make up one of the best *Festschriften* that has appeared in English history. McKendrick's prefatory essay is astonishing: a speech given at the dinner honoring Plumb on his retirement from his personal chair, funny and serious by turns, it is a far more frank and accurate assessment, both of Plumb and of his place in Cambridge, than is usual in such compositions. The final essay is Eric Stokes' inaugural lecture as Smuts Professor of Commonwealth history, but if it was not originally intended for this volume, it is in keeping in its arresting insights into its subject—the complex, symptomatic figure of Rudyard Kipling. The other eight essays, deliberately restricted to Cambridge associates and to English subjects, range from the sixteenth through the nineteenth centuries. If one seeks an underlying theme or approach among them, it lies—apart from E. V. Bennett's fascinating account of the precise way in which the fear of Jacobitism and Jacobite activity served the rise of Walpole—in the relation between thought (if one extends that to include assumptions and prejudices) and action, which may say more about a preoccupation of our time than of Plumb's direct inspiration.

In the classical mold of history of ideas are the elegant essays by J. P. Kenyon and Quentin Skinner, the first on the irrelevance of the steadily reiterated Whig theme of contract and resistance to royal oppression in the defense of the Glorious Revolution; the second on the "patriotic" posture adopted by Bolingbroke—a study in the deliberate, cynical adoption of accepted rhetoric that neatly cuts through the confrontation between Namier's (and others') dismissal of Bolingbroke and his admirers' defense of his intellectual commitment. A. Rupert Hall's judicious assessment of

the relationship (or, more properly, lack of it) between science and technology is a major contribution to a continuing debate and, like Skinner's article, directs attention to the *logic* of the argument. J. J. Scarisbrick analyzes the contents of and reaction to the Jesuit Robert Persons' detailed plans for the reconquest of Protestant England. Written in 1596 but not published for nearly a century, Persons' plans serve Protestant polemic "as one of the most influential unread books ever written." Taking up themes Plumb himself has dealt with, as do Bennett and Skinner, McKendrick examines the part played by the labor of women and children during the Industrial Revolution, not as a newly realized horror but as a major contribution to the growth of demand through increased family income. Barry Supple traces the patterns by which working-class thrift was encouraged through a variety of institutions and relevant government policy in the nineteenth century. And, finally, J. W. Burrow effectively demonstrates the differing uses of the concept of the village community by Maine, Freeman, Stubbs, and Maitland according to their responses to contemporary political challenges.

Each of the essays merits far more than a few compressed lines of description. They are all substantial contributions and, as rarely happens with the genre of the *Festschrift*, the whole is greater than the sum of the parts—a remarkable tribute to a remarkable historian.

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PAUL LANGFORD. *The Excise Crisis: Society and Politics in the Age of Walpole*. New York: Oxford University Press. 1975. Pp. 187. \$17.00.

Paul Langford has written a satisfying and much-needed study of one of the major political crises of the eighteenth century. The narrative is familiar, although there is some new material, but it is good to have in one volume the story of the excise bill and the election that followed it. Langford explains well each stage of the crisis: Walpole's decision to present the bill, the sources of resistance to the bill, the role of the court and the peerage in Walpole's defeat, the emergence of the bill as a major issue in the election of 1734, and the effect of the crisis on Walpole's government.

If there is a weakness in the book, it is that Langford does not bring out clearly enough the solidarity, persistence, and resourcefulness of the opposition in the parliamentary contest and the election. The strength of the book lies in its coverage of the role of public opinion. Initial opposi-

tion to the bill came from mercantile interests, but in the election of 1734 the striking feature was the intense hostility toward the bill in rural areas, where the excise was seen as an unwarranted extension of central authority into local and personal concerns. Langford's book is a useful contribution to our understanding of those court-country conflicts that erupted sporadically throughout the century.

Langford writes briskly, generalizes effectively, supports his statements with well-chosen illustrative material, and deals convincingly with a major topic in 179 pages of text and appendixes. Let us hope this volume heralds the end of the 300-page monograph, often nothing more than an inflated article crammed with every note the author ever took. If Langford's commendable economy is the result of the current crisis in publishing, we are the better off for it.

E. A. REITAN
Illinois State University

JOHN BARKER. *Strange Contraries: Pascal in England during the Age of Reason*. Montreal: McGill-Queen's University Press. 1976. Pp. xiv, 336. \$15.00.

This monograph describes Pascal's reputation in England from 1657, the date of the first English translation of the *Lettres Provinciales*, to 1844, the date of the first full French text of the *Pensées*. John Barker has examined a prodigious number of library catalogs, manuscript sources, and printed works, thereby uncovering all the important references to Pascal to be found in England.

Pascal was first read in England as an enemy of the Jesuits. Champions of Protestantism regarded him as an ally in the fight against popery and either forgot that he was a pious Catholic or assumed that he was a Protestant at heart. In the reign of Charles II, during the formative period of the Royal Society, he came to be greatly admired for his scientific and mathematical genius by Hooke, Boyle, Newton, and others. Finally, once the *Pensées* became known in England, Pascal enjoyed an enormous vogue as a religious philosopher. Here historians of ideas will note with interest that Englishmen of the period read an edition of the *Pensées* that contained less than half of what Pascal wrote and that omitted many of the most important passages.

Rationalists were both fascinated and repelled by Pascal's reflections on science and religion, reason and faith. Pietists and Evangelicals admired his emphasis on free grace and human depravity. The Romantics were interested in his religion of the heart.

Barker's work is tough to read: it is as if file after file of note cards had been carefully arranged un-

der topics and methodically reproduced. The book will be most useful as a work of reference. Unfortunately, the bibliography is divided into ten parts, and the reader never knows in which part to look. The reader has to leaf back and forth to find which notes refer to which chapters. A reputable publisher ought to know better!

RICHARD SCHLATTER
Rutgers University

JOHN B. OWEN. *The Eighteenth Century, 1714-1815*. (A History of England, volume 6.) Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xv, 365. \$12.50.

John B. Owen offers an unabashedly if attenuated "Namierite" political history of eighteenth-century England. There are appropriate references to Scotland, more extended discussions of political crises in Ireland and America, and occasional ventures into India. Four chapters on economy, society, culture, and the arts are sound, if highly compressed, summaries of the literature. These are slightly dated, sadly tangential, and almost irrelevant to the principal thrust of Owen's volume. The political narrative, however, is rich, firmly textured stuff. His acknowledged mastery of the world of Henry Pelham and the first duke of Newcastle gives Owen a refreshing perspective on the years between the fall of Walpole and the ultimate dissolution of the Old Guard Whigs. Chapter 5 is a splendid introduction to the subtle working of the eighteenth-century world of high politics, drawing on all that is best in recent scholarship. Chapter 12 is an intelligent, balanced account of how that pattern changed by the 1780s while laying proper stress on the elements of continuity. Although firmly in the Namier-Pares corner, Owen points to the merits of the more traditional "Whig" approach and presents, among other things, a particularly acute analysis of the Rockinghams and their evolution from Tong to Quasi-party. While ideas, even political ideas, are not Owen's long suit or principal interest, Edmund Burke certainly receives his due.

Owen's political narrative moves smoothly through the labyrinth of eighteenth-century dynastic foreign affairs and the rabbit warrens of oligarchal domestic factional squabbles. The detail and the subject matter will make this useful, but dense, material for the student, while Americans seeking a fuller background in English politics will find it a sound selection.

I have some serious reservations, most of which Owen addresses himself to in his introduction. For him history is essentially narrative. Too much analysis confuses rather than enlightens. Ideas had relatively "little influence on political or economic life" in this "oligarchic and pragmatic age" (p.

xii). Even in these terms, however, surprisingly little space is devoted to exactly how the political system worked and in what ways it involved people other than the charmed parliamentary circle. Owen barely touches on these points. There is a sprightly discussion of the borough system, some paragraphs on local government, but very little else except repeated general statements that the oligarchy continued to make itself the effective voice of a nation that, in at least half of the period covered, was undergoing dramatic change. "An oligarchy which could ensure hundreds of years of internal peace, establish an undisputed hegemony in Europe, acquire an Empire, preside over quiet revolutions in agriculture, industry and religion, and agree to share its power when the national interest so required, is deserving of respect, even of admiration" (p. 339). Every statement has a measure of truth, but it is unclear from this book exactly what sustained internal peace, whether it was the oligarchy or some select (usually eccentric) few among them who had the interest and capability to advance British hegemony in Europe and the world. It is even less clear what they did to further the agricultural, industrial, commercial, or demographic revolutions. What was the relationship, if any, between the oligarchy and the developments in religion, the British Enlightenment, and the evolution of romantic culture? Was the bulk of the oligarchy as oblivious to commercial development, trade, banking, and industry as Owen's presentation implies?

Culture seems painfully compressed. While Owen's themes are correct, the proportions are odd. Surely J. W. M. Turner, England's greatest modern painter, deserves more than one line. History deserves better from a historian. The Scottish Enlightenment receives short shrift. Even political thought, Viscount Bolingbroke's, for example, could have been enriched by a wider reading in the literature. It is perhaps appropriate that dilettante culture appears more significant than high intellectual and artistic achievement. That was, after all, the world of the oligarchy.

EUGENE C. BLACK
Brandeis University

JOHN FOSTER. *Class Struggle and the Industrial Revolution: Early Industrial Capitalism in Three English Towns*. With a foreword by E. J. HOBBSBAWM. New York: St. Martin's Press, 1975. Pp. 346. \$16.95.

In the first ten lines of his introduction Foster states briefly his purpose. His subject is the labor movement in Northampton, South Shields, and Oldham from 1790 to 1860. His "central theme" is the formation and decline of a revolutionary class consciousness in this period. His "basic aim" is to

improve our understanding of the development of industrial capitalism. The "main problem" to which he addresses himself is the change in the nature of English capitalism in the later years of his period. The adjectives central, basic, and main suggest some jostling among emphases, but the chronological pattern of the book mitigates it and assists the reader's understanding.

Generalizing primarily from the experience of Oldham (to which Foster devotes the most attention) in the 1790-1860 period, he finds three major changes in the social structure. Out of economic crises in the first thirty years a labor consciousness developed, while simultaneously the preindustrial authority systems collapsed. Next, a class consciousness formed locally, within industry, not as the work of an organized revolutionary party but by a "careful, conscious process" guided by "vanguard" radical leaders of an anti-capitalist persuasion. Then in the late 1840s a social restructuring began that Foster calls liberalization. Locally and nationally an establishment solution emerged as the will to oppose change weakened. A new authority system took form in which a new labor aristocracy participated. When made in these surroundings, concessions such as household suffrage appeared almost conspiratorial.

Foster's evidence, literary and quantitative and primarily local in origin, apparently shaped his description of the pattern of local history. Yet quotations from Marxist-Leninist scriptures seem to suggest influences that inform the use of evidence and facilitate leaps from evidence to conclusions that other persons might not be able to make so gracefully. Foster's discussions also suggest some personal involvement. He uses the word feudal in a pejorative sense (pp. 14-15). In asking whether certain developments helped produce an anticapitalist "mass perspective," Foster says that "the answer must be much less hopeful" (p. 42). In a certain respect, South Shields and Northampton "point in the right direction" (p. 107). These are loaded words—"hopeful" and "right" from a certain perspective. Influencing the inquiry by suggesting questions to be asked of the evidence and manners of addressing the questions, this perspective relates to other recent searches into the history of the Industrial Revolution, but the substantive quality of the evidence produced by Foster's research is unusually impressive.

CARL B. CONE
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RAYMOND CALLAHAN. *The East India Company and Army Reform, 1783-1798*. (Harvard Historical Monographs, 67.) Cambridge, Mass.: Harvard University Press, 1972. Pp. xiv, 242. \$11.00.

When the British lost much of North America in 1783, the possibility of losses elsewhere became a natural concern. Although the French threat to the British presence in India was checked, it had been a close call, and London assumed that when another European war broke out, there would be a renewed struggle in the distant subcontinent. Both the directors of the East India Company, whose enormous treasure was at stake, and the leaders of government, whose newly formed Board of Control now supervised policy for India, understood that everything depended on the company's Indian Army. That army's special practices and peculiarities, lucidly summarized by Raymond Callahan, were a source of anxious concern in London. Reform efforts begun in 1783-84 were still fruitless by 1793 when war with France made the matter even more urgent.

Despite fifteen years of determined effort by many powerful people, including George III and William Pitt, the reform effort failed. Callahan's account is both a good story and a good analysis. The reader will share the author's wonder that Cornwallis escaped serious censure, not only for the debacle at Yorktown, but again for disasters in India apparently caused by Cornwallis' mistakes. His successor in India, Sir John Shore, has commonly borne the brunt of criticism for the failures there during the 1790s, and Callahan succeeds in a moderate resurrection of Shore's reputation. The book does a superb job of explaining the astonishing ability of the Indian Army officers to preserve their perquisites, and perhaps even to improve them despite powerful efforts in the highest places to instigate change.

This illuminating account of one special subject is a small part of the work to be done on the story of the last hundred years of the East India Company's adventures. The authoritative writing that is pertinent needs a good deal of revision. Of some sixty entries in Callahan's bibliography of secondary works, over half were published before 1945 and about a third before 1900. More special studies are needed, and so is a larger account of the development of British India before 1858—a monumental achievement based almost completely on archive.

RICHARD D. MILES
Wayne State University

JOHN WOODWARD. *To Do the Sick No Harm: A Study of the British Voluntary Hospital System to 1875*. (International Library of Social Policy.) Boston: Routledge and Kegan Paul. 1974. Pp. xii, 221. \$17.25.

This small book (146 pages of text) brings together some useful data on the operation of voluntary hospitals in Great Britain during the eighteenth

and nineteenth centuries. The author introduces his work as a study "concerned principally with the contribution of the general voluntary hospitals in Britain to the health of the population."

Woodward then examines the converse, exploring whether or not the voluntary hospitals were "Gateways to Death" as charged by William Farr and Florence Nightingale. Within the limited and frequently controversial statistics of the period, using hospital death rates and the length of bed occupancy, he concludes that the hospitals achieved "what appears to be a remarkable degree of success in treating their patients and the mortality remained at a low level" (ten percent of admissions).

The book contains some interesting chapters on the characteristics of eighteenth- and nineteenth-century hospital nurses, on the types of surgery performed in voluntary hospitals, and on admission policies. The voluntary hospitals, which were funded by donations from the prosperous upper middle class and aristocracy, were established to serve the laboring, "deserving poor." Except for those injured in accidents or who were otherwise in acute conditions, a prospective patient of an English voluntary hospital needed a card or other recommendation from a contributor before admission to certify that he was a "proper object of charity." The chronically ill, maternity cases, and patients with apparent infectious disease were generally excluded.

It is obviously impossible to determine the relationship of any one type of medical facility to national health, given the multiplicity of biological, environmental, social, and economic factors that enter into the health of any population. But the merit of a study such as that by Woodward is that it extends the detailed, historical exploration of health care to which a number of British and American historians have contributed in recent years.

JEANNE L. BRAND
National Library of Medicine

PAUL HAYES. *The Nineteenth Century, 1814-80*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1975. Pp. xi, 334. \$16.95.

This study by Oxford Lecturer Paul Hayes is part of a five-volume series that will describe British foreign policy from 1485 to the present. The format is unusual. Following a discussion of the Vienna settlement, Hayes provides brief surveys of Britain's relations with the major powers, including Austria, Prussia, Russia, and France. Then he evaluates each of the foreign secretaries of the period from Castlereagh to Salisbury. Finally, he discusses Britain's relations with the minor pow-

ers, as well as the place of India and economic considerations in shaping foreign policy.

Adopting an attitude that so gravely offended Americans in the nineteenth century, the author relegates the United States to the obscurity of the last of the sections between disunited Italy and dying Turkey.

The discussions are uneven in quality. Hayes seems most at ease when describing events in Eastern Europe and least so when he takes up the New World. The section on France is disappointing. No treatment of Anglo-French relations during this period that virtually ignores naval technology can have much validity.

The author's evaluations of the foreign secretaries follow traditional lines. The strong nationalists, Canning and Palmerston, receive his accolades. The chief virtues of Lord Castlereagh, the internationalist, are said to have been depressingly humdrum—steadiness and hard work. Lord Aberdeen, of whose rational outlook the British should be justly proud, is depicted as weak and even stupid and mean-minded.

Many of the author's interpretations come from early twentieth-century authorities such as H. W. V. Temperley and C. K. Webster and are sometimes modified by the more recent contributions of Kenneth Bourne. Hayes raises no questions of interpretation in areas where scholarly controversies still exist, nor, indeed, does he even make the reader aware of them.

Hayes states that the work is not comprehensive, and it is perhaps all too easy to criticize any study that undertakes a project much too large for its physical dimensions. *Hoc opus, hic labor est*. As an introduction to the period, it serves its purpose.

WILBUR DEVEREUX JONES
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BRUCE MAZLISH, *James and John Stuart Mill: Father and Son in the Nineteenth Century*. New York: Basic Books, 1975. Pp. xii, 484. \$16.95.

ALAN RYAN, *J. S. Mill*. (Routledge Author Guides.) Boston: Routledge & Kegan Paul, 1975. Pp. xii, 283. Cloth \$12.50, paper \$6.25.

James and John Stuart Mill have long been sitting ducks for analysis. For some decades there have been occasional excursions into their unconscious by psychologists, literary critics, and historians. John Mill's cool, brilliant *Autobiography*, which Alan Ryan insists is a theoretical work and not "to be relied on as evidence about Mill's education and the effect of that education upon him," has nevertheless always been the starting point for any psychological inquiry into the formative influences on his mind and character. The presence of a

strong-willed father and the absence of a loving mother immediately call forth the lexicon of contemporary Freudian theory, which, when aided by developmental psychology, gives us the story of a life told in terms of oedipal and "Iphigenia" complexes, displacement, symbiosis, anality, latency, surrogate and intrusive parents, aggression, over-determination, and, to bring the account up to date, bisexuality.

Psychoanalytical explanations have long since entered our daily lives and to some extent have an accepted place in contemporary culture. It is one thing, however, to assess the emotional effect of a particular upbringing on the mind and life of a thinker, to attribute his political science to a psychic need for domination, his social theory to a need for liberty, and his economics to sexual expression; it is quite another to use psychoanalytic theory to explain whole transformations of thought and values in the historical life of societies. If Mazlish had stopped at the former, we might agree that his account of the Mills and Harriet Taylor is interesting and certainly helps shape our picture of their relationships. We would at the same time have to bear in mind Ryan's more conventional but no less convincing explanations of some of the same events—for example, John's famous breakdown and crisis. Mazlish is of course after bigger game. His psychohistory seeks to explicate the whole industrializing or modernizing process itself, and he uses Freudian theory to understand how new character types were created. He is in some respects following in John Mill's ethological footsteps. It is possible, he asserts, to see constants such as our psychic nature uniting with changing social and intellectual circumstances to produce wholly new sequences of behavior and thought.

This statement seems unexceptional, and the outlines of Mazlish's general argument are certainly well known. The characteristics of industrialization or modernization are work, achievement, a new time discipline (all of which are intimately connected to Protestant individualism), the rise of the self-made man, the alienating effect of modern economic systems, the rise of the conjugal family, the child-centered family and adolescence, Victorian sexual repression, the subjection of women, and what Carlyle called a "diseased introspection"—the evangelical emphasis on sin and guilt. To these broad and familiar ideas Mazlish adds the interpretive support of psychohistory; the result is an elaboration of the ideal-type capitalist given to us by Weber, which has always been as much a psychological construct as a sociological one.

The advantages of modernization theory are not at the moment apparent. There are problems of chronology and weighting. Is the modernization

process mid-seventeenth, early eighteenth, or early nineteenth century? Are religion and education as important as industrialism? There are major disagreements about when supposedly typical or decisive character traits first become visible or dominant. The interplay between financial capitalism and industrial capitalism, and between a profit-oriented English landlord stratum and a group of entrepreneurs engaged in overseas trade, has yet to be worked out in terms of modernization assumptions. Sociological definitions like "class," especially in connection with consciousness, lack a clear historical formulation. I wish Mazlish had not so quickly converted James Mill's "middle rank" into "middle class" or been quite so positive that "educated" means middle class and "rich" means upper middle class. The problem of the appearance of the conjugal family—where we first encounter it and among which groups it first appears—is still vexed, as is adolescence. The relationship between husbands and wives in all social groupings and in all historical periods is also up in the air. New evidence may well point to a far stronger role for mothers in child rearing than the study of James Mill and his wife indicates. In order to make sense of modernization, we have to get all the right variables together at the right historical moment; if we cannot, perhaps our assumptions and objectives should be re-examined.

Problems of chronology, labeling, and evidence appear throughout Mazlish's book. There is the large question of whether words that today convey explicit or implicit Freudian meanings could have carried those same meanings in the past. Gandhi cannot be used to define Georgian words like "manly," which is too readily given a psychosexual meaning here, and "Man," as it appears in Enlightenment philosophy, should not be emphatically declared masculine. We should not be told that Harriet Taylor's father, born in the eighteenth century, is Victorian, nor should we be left with the understanding that James Mill is Victorian as well. The result of this sleight-of-hand is to give us a whole series of supposedly Victorian traits and characteristics—the patriarchal family, the "strict and joyless tenets of Puritanism," the evangelical conception of the child as sinner, and the alleged irreligiosity of urban England as opposed to preindustrial, rural England. All of these hardly do justice to either the Victorians or the Georgians. Anyone who has worked through late eighteenth- and nineteenth-century Anglican theology will hesitate to say that "orthodox Christian religious conceptions" easily reduce to the idea of an all-powerful father-God. It is time for theologians to come to the aid of secular historians and psychologists. We have labored too long in overconfident isolation.

The student of English history will find other assertions and errors to contend with. Why, for example, are the electors of Westminster in the pre-1832 period referred to as if they were £10 householders when in fact the franchise was "pot-waller," that is, virtually democratic? Why is dependency-independency made a special problem for nineteenth-century industrial or modern man, and the need for personal autonomy related to "wider ideological needs, corresponding beautifully with the Utilitarian stress on individualism," when the entire eighteenth century was filled with unbeneficed clergy, disappointed Oxbridge fellows, ambitious artists, and frustrated young men of ambiguous origins who constantly decried the system of aristocratic patronage? There is no need to insist that James Mill, the proud young man forced to toady to the great, is somehow once again the harbinger of the future. Mazlish compares him to Stendhal's Julien Sorel, who "may be considered the prototype of what, along with the governess, was one of the major social types of the time." What happens to the point that the introduction of age-grading makes achieving more conscious when the evidence cited is Jeremy Bentham, who "at age twelve was not unique in being at Oxford University in the same class as a twenty-four-year-old"? As a matter of fact, he was unique. Only four percent of Oxford matriculants in 1760-61 were fifteen years of age or younger.

There are times when Mazlish is aware that his timing may be off or that his generalizations about family development are a bit unsteady, but his eagerness to find England in a fully industrial condition in 1800 carries all before it. He is too anxious to push conventional Victorian stereotypes back to 1800. He is too eager to employ psychoanalytic concepts in support of a modernization model that has long been available but has never been altogether conceptually or factually persuasive. He is undoubtedly correct in thinking that psychohistory will have a safer future when it ceases to claim a dominant or causal role in historical explanation and when the psychic life is viewed more modestly in relation to other biological or cultural factors. While historical phenomena or social relations can be theoretically reduced to psychoanalytic mechanism, we cannot reverse the process and reason our way to particular social and institutional structures from psychological data. Psychohistory, in facing up to this, will not receive the cold shoulder to which Mazlish refers in his opening pages. If the reader wishes to have a guide through the many writings of the Mills, especially John, that takes cognizance of inherited traditions of thought and offers as well some cautious advice on how to read the *Autobiography*, he

will peruse Ryan's successful contribution to the Routledge Author Guides.

SHELDON ROTHBLATT
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PHILIP ROSENBERG. *The Seventh Hero: Thomas Carlyle and the Theory of Radical Activism*. Cambridge, Mass.: Harvard University Press. 1974. Pp. xi, 235. \$10.00.

Are wives and husbands (dog-owners and dogs) attracted to one another because they are alike, or do they become alike because they are attracted to one another? It is a puzzle as great as the parallel one concerning authors and what they choose to write about. In any case, Philip Rosenberg shows in his study of Carlyle an arrogance and a contempt worthy of his subject.

With Podsnappery he states that Carlyle's early essays have "little intrinsic interest," while his later writings seem "scarcely worth reading and even less worth writing about." But the "certain sadness" to which Rosenberg "must confess" over his confining Carlyle's valuable work to the decade and a half ending with *Past and Present* (1843) need not bring the reader to melancholy madness, for he is quickly reassured "that rescuing fifteen years of Carlyle's career from oblivion is no disservice to his achievement." The tone here established leads unsurprisingly to the statement that "one of the primary purposes of this volume" (the others are not delineated) is "to demonstrate that Carlyle's writings, if properly interpreted, can be of considerable value in the development of a theory of radical activism at a time when, after the diffuse and unfocused activism of the 1960s, such a theory is badly needed.

The key to the whole work is that parenthetical phrase, "if properly interpreted"; Rosenberg assumes a rare hermeneutic authority over the history of ideas as well as over Carlyle's utterances, and with considerable sleight of hand he pulls from Carlyle's hat rabbits of uncertain ancestry and questionable vitality. The "might is right" debate, for example, arises because "liberals" (contractualists all) fail to recognize that the assertion of identity of might and right is a mere truism; those who have power have power, and all moral considerations are as dust beneath the crushing wheels of pragmatism. Similarly, Rosenberg, by equating the "rationalist-idealist side of the political spectrum" with "liberalism," can leave "realism" (concerning historical models) as the equivalent of radical Carlylianism—or almost anything else.

I must confess to a certain sadness that I discovered so little of the value discerned in the work by

others (most notably, Barzun and Trilling on the dust jacket). There is nothing new here about Carlyle, except the revisionism that insists on his special relevance to the 1970s; there is much that is mistaken about the nineteenth century; and there is considerable evidence of cleverness and commitment. As Max Beerbohm is reported to have said, "This is the sort of thing you'll like, if you like that sort of thing."

JOHN M. ROBSON
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A. J. MEADOWS. *Science and Controversy: A Biography of Sir Norman Lockyer*. Cambridge, Mass.: MIT Press. 1972. Pp. ix, 331. \$16.95.

From scientific controversy comes either new theories or affirmation of established ideas. The loser, in this case Sir Norman Lockyer, carries nothing away unless it is the satisfaction of having attention focused on his research, even though his results are rejected. A. J. Meadows believes that the "losing" scientist provides a useful perspective on his more successful contemporaries; he is a mirror of the beliefs of his time.

Sir Norman Lockyer was a self-educated scientist in an age when that was both possible and admirable. A pioneer in solar physics who developed important techniques for studying the sun during eclipses and at other times, he discovered helium through spectroscopy. He was also the founding editor of the British journal *Nature*. These activities spanned the period from 1860 to 1920.

The story of failures gives a seldom-used insight into scientific enterprise. In a scientific controversy, such as the one over Lockyer's dissociation hypothesis, the standards for verification are revealed and the growth of scientific ideas better understood. But in describing this as well as other controversies, Meadows devotes more space to the personal jousts than to the issues involved. Lockyer appears pitiful and frustrated, an intractable rather than an original thinker who was wronged by the conservatism of his time. His opponents are vindictive in denying him the gold medal of the Royal Astronomical Society and keeping his son out of the Royal Society. The seamy side of science is exposed, and Lockyer is seen as less heroic than picaresque.

Can this biography be the true picture of Sir Norman Lockyer, an important scientist during an era when British science was supreme? It will take another biography to unveil the Lockyer who was a positive force in Victorian science and society.

HAROLD ISSADORE SHARLIN
Iowa State University

DAVID MORGAN. *Suffragists and Liberals: The Politics of Woman Suffrage in England*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. vii, 184. \$12.50.

ANDREW ROSEN. *Rise Up, Women! The Militant Campaign of the Women's Social and Political Union, 1903-1914*. Boston: Routledge and Kegan Paul. 1974. Pp. xix, 312. \$21.75.

A prime requisite in a modern historical work is a rational explanation of an event in the past, reason being the historian's basis of both comprehension and communication. A controversy such as that over women's suffrage, in which attitudes were determined as much by emotional assumptions as by rational considerations, if not more so, creates a problem that taxes the ingenuity of the writer to make sense of the irrational for himself and his readers. All those involved, male and female, for and against, had motivations not always rational and not always acknowledged though frequently rationalized. To fail to recognize these complexities when discussing the volatile issue of votes for women is to distort the stuff of politics and to give the appearance of stupidity or double-dealing to those pulled in one direction by reason and another by prejudice.

Andrew Rosen in *Rise Up, Women!* faces the problem by adhering closely to a detailed chronological account of the activities of the suffragettes that fulfills his claim to be "faithful not only to the facts as [he] found them, but faithful in affect as well— . . . evocative as well as accurate." By evoking the atmosphere, he makes irrational actions and reactions comprehensible. David Morgan seems unaware of the problem in *Suffragists and Liberals*, a study of liberal party politics and the women's suffrage question between 1906 and 1918, and he gives almost no weight to the irrational motives that affected the decisions of both male and female protagonists. He accepts the words, especially of the politicians, at their face value and seeks to present explanations based solely on rational political considerations for subsequent dilatory behavior.

Morgan's book has other irritating flaws that probably stem from this limitation and certainly reflect it. His idiosyncratic decision to use the term suffragist for supporters of both the Women's Social and Political Union and the National Union of Women's Suffrage Societies is at the very least confusing and, as in the title of the book, sometimes misleading. To speak throughout of "the campaign for what was called Woman Suffrage" by that name, which was felt to have pejorative connotations, reveals an insensitivity to, or ignorance of, the feelings of the participants who accepted John Stuart Mill's insistence on the term "women's suffrage" rather than "woman suf-

frage," the latter term then being most usually adopted by opponents of the women's movement. Other remarks such as, "Besides the obvious George Eliot there were polemicists such as Mrs. William Ellis who was not really a feminist writer," further indicate Morgan's lack of understanding of the movement and its aims, and of the values that men and women believed were at stake in the controversy symbolized by the demand for the vote.

Rosen, on the other hand, is always well aware of the charged atmosphere that surrounded the women's struggle. His exhaustive and meticulous search through the primary sources has not dulled but heightened his awareness of the angers, frustrations, and prejudices on all sides that created the atmosphere in which the irrational flourished. His interest in the movement was broadened by his realization that the "exasperation of the suffragettes at the apparently complete inefficacy of conventional methods of agitation" drove them to the extreme phase of militancy that "came to resemble, in certain aspects, millennial movements of other eras."

Both authors wisely refrain from attempting a dogmatic answer to the vexed question of whether the extremism of the final years before the First World War retarded the achievement of the vote for women, although Morgan accepts militancy as one of the reasons for procrastination. Reading *Rise Up, Women!* and *Suffragists and Liberals* confirmed my own feeling that to retard the consummation of a fifty-year-old campaign was at worst a semantic achievement and that the suffragettes may well have provided that advanced position for which Jessie Boucherette praised John Stuart Mill's *The Subjection of Women* in 1869: "There is a great body of people who endeavour to keep in the middle avoiding the two extremes of opinion as they think and I observe that the further the most advanced opinions go, the further they go. . . . They don't feel safe unless there is a body of opinion before them. They then decide in a judicial manner and looking immensely wise that there is something to be said on both sides but that the extreme of opinions on both sides are wrong."

ANN ROBSON
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JOHN E. KENDLE. *The Round Table Movement and Imperial Union*. Buffalo, N.Y.: University of Toronto Press. 1975. Pp. xvi, 332. \$17.50.

The Round Table was the most important and most intellectually significant of the groups working for federation among the states of the British Empire in the early decades of this century. It had its inception among the group of young men who

formed Alfred Milner's "kindergarten" in South Africa after the Boer War. Inspired by their success in forwarding political unity there, they sought further triumphs in working for the creation of a form of organic unity among the nations of the Empire.

They attempted to accomplish this by creating discussion groups recruited from the ranks of sympathetic intellectuals and opinion makers in Britain and the major dominions. The members of the Round Table, however, were a singularly ethnocentric group. As John Kendle indicates, they were drawn from the Anglo-Saxon Protestant middle class, though the British participants had some aristocratic underpinnings. Through the pages of their journal and other contacts, like spoke to like across the waters. There was much preaching to the righteous but little effort to call sinners, such as Bourassa in Canada or the leaders of the Australian Labor party, unto repentance. For all of their imperial concerns, they seldom deigned to submit their ideas to the marketplace forces of political debate, and, as the author suggests, had they done so they would have met with little support. In the development of imperial relations, the Round Table groups were essentially side shows, clever and attractive in some ways but side shows nevertheless.

While the general story of the Round Table is well known, Kendle has fleshed it out with skill, using widely scattered sources. He has perhaps focused his efforts somewhat narrowly. His account might have been enriched had he discussed in greater detail the differing measures of acceptance or rejection given the movement in the various states of empire; but the work as it stands is a contribution to the understanding of an important aspect of British Empire-Commonwealth development.

DONALD C. GORDON
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A. C. H. SMITH *et al.* *Paper Voices: The Popular Press and Social Change, 1935-1965*. With an introduction by STUART HALL. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. 262. \$12.50.

A product of research conducted at the Centre for Contemporary Cultural Studies at the University of Birmingham, this study is an effort to use the approach to popular culture advocated by the Centre's former director, Richard Hoggart. The type of literary and linguistic analysis ordinarily reserved for works of "high culture" is applied to the *Daily Mirror* and the *Daily Express* during the election campaigns of 1945, 1955, and 1964. Vocabulary and images are examined to discover the

differing assumptions that the two newspapers made about their readers and the contrasting pictures of society that each implicitly conveyed.

While some of the findings may appear self-evident, this systematic investigation gives hard evidence to support what hitherto had been simply impressions. During the war the *Daily Mirror* evolved a "stylized working class speech" that associated it with its audience, the ordinary Britons, against the privileged "Them." Encouraging correspondence from its readers, its staff sensed the collectivist mood that was developing, and thus recognized, as the *Daily Express* did not, that such issues as housing would be important factors in the 1945 election. Amid the affluence of 1955 the *Mirror's* "mode of address" was inappropriate, and the *Daily Express*, which had throughout pictured the nation as individualistic and hierarchical, was in closer harmony with the public mood. The study concludes that newspapers are neither simple reflections of their society nor powerful independent influences on it. Rather, those newspapers that interpret social change in terms "congruent" with their readers' experience will reinforce existing trends; those that do not will have little effect.

This work is of interest less for its conclusions, however, than as a demonstration of the limits and possibilities of its methodology. As the author recognizes, his premise that the personality of a newspaper is a product of unconscious assumptions rather than of deliberate policy can only be established in conjunction with more traditional studies based on the papers of editors. Nonetheless, by pointing out the inferences that can be drawn from a newspaper's choice of language and of images, he has added a new dimension to the study of the press.

CATHERINE ANN CLINE
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MARTIN GILBERT. *Winston S. Churchill. Volume 4, 1916-1922: The Stricken World*. Boston: Houghton Mifflin. 1975. Pp. xvii, 967. \$27.50.

The sheer scale of this monumental study is staggering. Less than half of its documents come from the Churchill Collection; the majority, including many of Winston's important private letters, are found in such wide-ranging sources as the Margot Asquith Papers and the Shane Leslie Papers in private possession, the H. A. L. Fisher and T. E. Lawrence Papers at the Bodleian, the Samuel and Weizmann Papers in Israel, the Loucheur Papers at the Hoover Institution, and the Colonel House Papers at Yale. In quite readable, if uninspiring prose, Martin Gilbert meticulously elaborates six of the most complex years of Churchill's life.

This volume spans the period from December 1916 to November 1922. They were the years when Churchill returned to the corridors of power after the Gallipoli disaster to serve as a member of Lloyd George's cabinet, first as minister of munitions, then as secretary for war and colonial secretary. Among the many challenges Churchill confronted were the demobilization of the armies in January 1919, the British intervention against the Bolsheviks in Russia, the civil war in Ireland, and the innumerable problems in Britain's new Middle Eastern territories.

Several engaging chapters of the forty-eight in this volume are entitled "Mr. Churchill's Private War," "Palestine 1922: Defending the Balfour Declaration," and "The Mysterious Power of Ireland." In the first chapter and in two others on the intervention in Russia, Churchill's fanatical hatred of bolshevism is colorfully portrayed. He told the House of Commons in November 1919 that the Germans sent Lenin to Russia "in the same way that you might send a phial containing a culture of typhoid or of cholera to be poured into the water supply of a great city," to which A. J. Balfour retorted to Churchill: "I admire the exaggerated way you tell the truth." Churchill defended General Denikin until the bitter end.

There was a curious conjunction of imperial, religious, and racial attitudes during the Commons debate on the Amritsar Massacre in India. In defending the conduct of General Reginald Dyer, "a gallant officer of 34 years' service" who had ordered his troops to fire on an unarmed crowd killing over three hundred Indians, Sir Edward Carson, the father of Northern Ireland, capitalized on the anti-Semitic feeling aroused by Edwin Montagu, the Jewish secretary of state for India. Montagu had set up the government commission that condemned Dyer's actions, and so Montagu was attacked by many right-wing Tories for, in Carson's words, his "un-English" conduct in the case. Churchill delivered the crucial speech, winning over the House by characterizing the massacre as "an extraordinary event, a monstrous event, an event which stands in singular and sinister isolation." He proposed the sane doctrine "that no more force should be used than is necessary to secure compliance with the law."

As colonial secretary, Churchill attempted a very sane policy toward Ireland, but was puzzled by the "mysterious power" of that "small, poor, sparsely populated island" to topple British governments and bring the country to the edge of civil war in 1914. He did not want to abandon Ulster, but he did not want Ulster to be a barrier to a treaty with the "Irish Free State." He maintained that it was "our policy" to encourage Ulster "in her own time" to unite with "Southern Ireland";

in the meantime Britain had accepted "a complete obligation for the defence of Ulster." When civil war broke out in Ireland after ratification of the treaty in January 1922, Churchill told the Commons that "one would have to search all over Europe to find instances of equal atrocity, barbarity, cold blooded, inhuman, cannibal vengeance—cannibal in all except the act of devouring the flesh of the victim." Ireland therefore even defeated Churchill. Lord Hugh Cecil, leader of the diehards, remarked to him bitterly but accurately about the Irish treaty and agreement: "Your statue is a statue of snow, which will dissolve under the sunlight of reality."

We occasionally get a glimpse of the private Churchill, especially in 1921 with the unexpected losses of his mother and his youngest daughter, Marigold. He ironically wrote that "another twenty years will bring me to the end of my allotted span even if I have so long. The reflections of middle age are mellow. I will take what comes." Clementine spoke strongly to her husband about his character when he was in France on a mission in 1918 as minister of munitions: "I would like you to be praised as a reconstructive genius, as well as for a Mustard Gas fiend, a Tank juggernaut & a flying Terror."

It is unfortunate that Gilbert's prodigious effort in amassing sources is not matched by a skillful editorial job. This book is supplemented by a mammoth companion volume of sources in three parts or volumes, making the lack of selection and condensation especially unfortunate. For the years covered in the volume under review, one page is devoted roughly to every sixty hours of Churchill's life. One shudders at the prospects for 1940 to 1945. Gilbert makes little use of secondary scholarly works on the period, and the book often reads like the worst Victorian biographies, a collation of primary sources—many of them fatuous or repetitive. Churchill deserves a well-written and thoughtful, if controversial, synthesis, not a monumentally boring compilation.

JOSEPH M. HERNON, JR.
*University of Massachusetts,
Amherst*

FRANÇOIS LEBRUN. *Les hommes et la mort en Anjou aux 17^e et 18^e siècles: Essai de démographie et de psychologie historiques.* (Ecole Pratique des Hautes Etudes—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, number 25.) Paris: Mouton. 1971. Pp. 562. 88 fr.

Working in the *Annales* tradition, François Lebrun has added to our knowledge of a region of western France that did not undergo the same changes as

the rest of the kingdom before the Revolution. Anjou's population stagnated during the eighteenth century. Although its linen and canvas industries showed signs of a new vitality after 1750, they declined shortly thereafter. The enlightened efforts of royal administrators and local agronomists like the marquis de Turbilly to expand land cultivation had only limited success because the Angevin peasantry clung stubbornly to outmoded agricultural methods. Most important, despite the hospital reforms of the Catholic Reformation and the new empiricism of medical pioneers like the Hunaulds and Michel Chevreul, in the eighteenth century, hospital facilities remained woefully inadequate, and medicine continued to be practiced by swarms of surgeons (*internes* and *externes*), *sages-femmes*, *empiriques*, and even parish priests upon whom the new medical methods had little, if any, influence. Improved royal administration during the eighteenth century made possible more effective help in times of epidemic, but the death toll from disease remained high among artisans and peasants sunk in a chronic state of misery.

Lebrun divides his study into three sections. In the first two he charts demography and makes estimates of mortality rates. Although he does a commendable job in overcoming a tremendous handicap caused by fragmentary source material, he does not marshal his diffuse data to determine the precise nature of the relationship between crises of subsistence and periodic aberrations of climate, nor does he investigate the connection between crises of subsistence and outbreaks of epidemics.

In the third section he provides fascinating information about the differing mentalities of parish priests who considered illness a divine warning or punishment, *empiriques* and conjurers who treated illness by utilizing magical powers, and doctors who denied the supernatural character of illness, thereby incurring the suspicion of the Angevin peasants. His research substantiates the thesis of his mentor Pierre Goubert that popular religious belief merely cloaked an elaborate set of deeply ingrained pagan superstitions. But when he turns to such eighteenth-century developments as the decline of the custom of interment in churches and the decrease in the number of legacies left to religious orders, he deals much too briefly with the complex subject of the profound transformation of Angevin attitudes toward death. If death remained omnipresent in Anjou during the eighteenth century, as Lebrun's research indicates, why did it lose its centrality to the Angevin outlook? Certainly Lebrun's explanation of this apparent paradox in terms of the waning of the Catholic Reformation in Anjou is unsatisfactory after the work

done by John McManners calling attention to powerful secular forces of reform. In spite of these reservations, Lebrun's study of Anjou is important because it points the way to research yet to be done.

RONALD I. BOSS
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DALE VAN KLEY. *The Jansenists and the Expulsion of the Jesuits from France, 1757-1765*. (Yale Historical Publications, Miscellany, number 107.) New Haven: Yale University Press. 1975. Pp. x, 270. \$15.00.

In this study of the complex events leading to the expulsion of the Jesuits from France, Dale Van Kley has offered a perceptive and important contribution to our knowledge of the political and cultural history of eighteenth-century France. Never have the byzantine interactions of this affair been explicated with such surety and evenhandedness.

In part, Van Kley's success grows out of his careful attention to archival materials. His striking addition to our archival knowledge of the event—full use of the papers of Louis Adrien Le Paige, an influential Jansenist lawyer in the Parisian Parliament—has allowed him to reconstruct the politics of the expulsion with a high degree of certitude. He is careful not to go beyond his evidence, yet he is able to offer a convincing model: that despite the variety of motives which contributed to the expulsion, a small, close-knit, theologically serious party of Parisian *parlementaires* and parliamentary lawyers orchestrated events in a self-conscious conspiracy.

In large measure Van Kley succeeds because of an admirable sense of how political processes work. Eschewing one-dimensional models—for example, that the expulsion represented simply Gallican defiance—he is careful to delineate the displacements and curious alliances allowing a small activist party to capitalize on such a seemingly trivial event as the La Valette affair. Van Kley clearly delineates how Jansenists became fixated with the Jesuits, how Gallican and Jansenist ideas were mated without either destroying the other, how the Jansenist party—which after the expulsion became furiously antiphilosophic—welcomed the collaboration of an apparent secularist such as La Chalotais, and so on. He brings to his account a fine sense of the ironies of political events and, important in a story of this complexity, a highly readable style.

If there is a weakness in the book, it lies in the conclusion he draws as to the role of Jansenism in the Enlightenment—that the success of the Enlightenment in France by 1789 must be in part

attributed to its becoming "more Jansenist" (p. 237). To be sure, as Van Kley points out, the philosophes capitalized mercilessly on the Jansenist success, and no doubt they also picked up on some of the antidespotic and curiously anticlerical language of the Jansenists. But this hardly seems proof of a fundamental interaction; rather it points to the variety of the philosophes' tactics and the multiple sources of constitutionalist doctrine in the eighteenth century.

Still this minor criticism should not detract from what ought to be the central judgments on the book—that it is an admirable contribution to the study of eighteenth-century France and early modern politics, and that it ought to provide a model for the further development of the political history of the period.

HARRY C. PAYNE
Colgate University

GABRIEL ARDANT. *Histoire de l'impôt*. Book 2, *XVIII^e et XIX^e siècles*. (Les grandes études historiques.) [Paris:] Fayard. 1972. Pp. 870. 60 fr.

Gabriel Ardant uses taxation as a frame in which to portray the economic development of the West and the role of government within it. Volume 2 of his *Histoire de l'impôt* is a long and detailed account of the development of taxation from the end of the seventeenth century to the European Economic Community of today, focused mostly on France, but touching Britain and the European continent at many aspects and extending at the end to the USSR and China. The story is brought from taxes in kind, in services and the *corvées*, to money payments; from tax farming to centralized collection; from tithes, *dîmes*, capitation levies, taxes on windows, hearths, doors, and land especially surveyed cadastrally for the purpose, to levies on commerce; from taxation primarily within nation-states to that between them; and from salt and bread taxes to those on wine, tobacco, coffee, tea, and cocoa. The income tax appears at the turn of the eighteenth century in Britain and with the Third Republic in France. Indirect taxation is streamlined in the twentieth century as excise and sales taxation is replaced by the elegant tax-on-value-added, which conforms to Ardant's criterion of neutrality as opposed to taxation that is distortionary.

Gunnar Myrdal once said that France is the country with the best professors of public finance and the worst national accounts. Ardant claims great importance for public finance, especially in war and recovery from war, and credits British successes in the Queen Anne's and Seven Years' War to the superior ability of the crown to extract

revenue from its subjects. The European enemies of France were unable to fight without subventions. Mistakes of taxation led to the American and French Revolutions; Pitt's income tax defeated Napoleon at Waterloo. There is something to the point, but it is easily exaggerated.

Ardant misses an opportunity to pursue the economic aspects of foreign policy when he ignores the public finance side of the *Zollverein*. A new paper by Rudolf Dumke notes that the elimination of thousands of miles of tariff boundaries among the members of the *Zollverein* drastically reduced heavy costs of customs collection, to constitute a real saving for all. In addition, the technique adopted by Prussia of dividing customs revenue by population in effect bribed the princes of the states along the southern tier to join by guaranteeing them income raised without necessity to levy unpopular taxes. Nor does he contemplate the possibility that Holland's failure to industrialize at the time of the British Industrial Revolution was the consequence of the mercantile oligarchy's insistence on taxing the cost of living of the working classes, rather than foreign trade, thereby raising wages and making industry uneconomic, as a recent thesis by Joel Mokyr at Yale contends.

Main sections of the book are devoted to the eighteenth century up to the American and French Revolutions, to revolutions themselves, and finally to the Industrial Revolution. These are introduced by extended bibliographical sections, devoted largely to the French literature in the field. The use of the book for reference is handicapped by lack of an index.

CHARLES P. KINDELBERGER
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FELIX MARKHAM. *The Bonapartes*. New York: Tapplinger. 1975. Pp. 224. \$14.95.

Felix Markham has set himself a formidable task in dealing, in slightly more than a hundred pages of text, with all the Bonapartes from their Corsican origins to the present day. Such a book could not be expected to contain anything really new, and it will appeal to the layman rather than the historian.

Unfortunately, the attempt does not succeed as well as it might have. The volume contains many trivia. Many of the numerous inaccuracies could easily have been eliminated with a little less haste in writing and a little more care in editing. Borodino was fought in 1812, not 1811; Napoleon did not get to England in the winter of 1806 (p. 134). In addition to such slip-ups, there are several errors of statement, some because of the need for condensation. For example, the second coalition was not formed because of Nelson's victory at Abukir

Bay. The author also makes definite statements about matters historians are still disputing.

There is another kind of difficulty. Felix Markham, who has written two earlier books on Napoleon, knows the subject so well that he does not furnish the uninformed reader with necessary identifying material. He refers to "the coup" or "the telegram"; the reader might not know that Louis Napoleon carried through a coup, or that there was an Ems dispatch.

But the book has good points. It ties together the various threads of the Bonaparte fabric. The material on the fate of the family after Napoleon's fall is interesting. Throughout, the author serves up tidbits. There is a genealogical chart, which takes a little puzzling over because of its form; a bibliography of approximately two dozen secondary works, mainly popular; and an index. But above all, there are over a hundred pages of fresh and felicitous illustrations.

This work will not enhance the author's scholarly reputation; it might well enhance his income tax.

ROBERT B. HOLTMAN
Louisiana State University

JEAN-PAUL ARON *et al.* *Anthropologie du conscrit français, d'après les comptes numériques et sommaires du recrutement de l'armée (1819-1826): Présentation cartographique.* (École Pratique des Hautes Études—Sorbonne. VI^e Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, number 28.) Paris: Mouton. 1972. Pp. 262. 85 fr.

This volume summarizes and analyzes military recruitment data primarily for the years 1819-26 and mostly in the form of maps and tables, supplemented by introductory and concluding essays. It examines the conscripts, classifying them by stature, vocation or profession, literacy, and the infirmities or deformities that excused some from service. Nineteenth-century military administrators used a system of classification that often does not respond to the twentieth-century historian's questions. Thus the volume lumps various kinds of urban and industrial workers together into an "other" category, while noting individually the numbers of shoemakers, tailors, masons, and carters—men whose skills would be immediately useful to the army.

Nearly half the volume consists of departmental maps of France indicating in percentage terms the number of men possessing a particular occupation or excused from service by a specific disease or deformity. The maps use the Bertin method whereby each department contains black circles or dots, the size of which corresponds to the percent-

age of the category present. This method permits comparison within a category, but since the scale varies, one cannot usually compare one page with another. For example, the maps indicating the number of workers in iron and other metals and the number of laborers look superficially similar in terms of the amount of darkened space, but the former ranges no higher than five percent of the departmental class, while the latter ranges as high as eighty percent. Most of the maps are clear and understandable, but one concerning the ability to read must surely be incorrect or mislabeled, because its figures are inconsistent with the other maps conveying the level of literacy.

As Dumont and Le Roy Ladurie note, the study contains few surprises. Northeastern France supplies young men who tend to be taller and more literate than those of the rest of the country. Most diseases and deformities follow no consistent geographical pattern. Occupations in agriculture are most common in the underdeveloped southern and western departments. Jean-Paul Aron's essay in "anthropological history" discusses the operation of the medical councils that examined conscripts. Aron struggles vainly to find significant relationships between height and disease, and he concludes with random comments on attitudes toward size, sickness, and race.

The volume exemplifies a danger of quantitative methodology, presenting data of limited scope and significance with clarity and detail, but failing effectively to relate the information to the larger issues of nineteenth-century French history.

JAMES F. TRAER
Hamilton College

SHERMAN KENT. *The Election of 1827 in France.* (Harvard Historical Studies, volume 91.) Cambridge, Mass.: Harvard University Press. 1975. Pp. xi, 225. \$12.50.

After Waterloo, the Revolutionary and pre-Revolutionary notables throughout France engaged in a bitter civil war that raged in government offices, in private salons, in the press, and especially in the Chamber of Deputies until the 1830 Revolution finally resolved the matter by force. Sherman Kent's *The Election of 1827 in France*, a triumph of classic historical scholarship, is the story of one crucial battle in that fifteen-year civil war.

Kent brings to light important new material. He is the first to use Villèle's revealing diary, and he has found in the archives of sixty departments both reports on local opinion and ministerial circulars not preserved in Paris. He shows convincingly how the Villèle ministry, which tried to work for the interests of the pre-Revolutionary notables within

the new constitutional system, was betrayed rather than defeated in the 1827 elections. Kent's revised statistics show that the government would have won the elections, both in the number of ballots cast (appendix III C and page 170) and in the number of deputies elected (appendix III B and pages 161-70), if the far right had not joined the left in a "monstrous alliance" against Villèle. Furthermore, the local prefects found the right-wing Friends of the Freedom of the Press a more effective enemy than the much-praised *Aide-toi, le ciel t'aidera*, dominated by the liberal revolutionary-imperial "new men" (pp. 81-88, 170).

Kent's new material gives us a sharper picture of the Restoration. He shows us (pp. 7, 15-16, 35n., 41, 55, 58, 159-60, 188-89) how Charles X appears in Villèle's diary as a conscientious and concerned monarch, agonizing over each decision, determined to lead his nation. In the correspondence of the prefects, we see the ministry's concern for the rule of law taking precedence over its will to win (pp. 130-56, 85-86n., 77, 78, 115, 128) and, among other things, allowing 18,000 voters to be added to the prefects' first electoral lists, mainly through the efforts of the two oppositions (pp. 59-61, 116-20). Kent points out how the constitutional, parliamentary government of the Restoration had awakened the political pulse of a nation that had become cynical during the "absurd electoral ballets of Napoleonic times" (p. 105).

The 1827 election doubled the representation of businessmen (to fourteen percent) and tripled that of professionals (to eleven percent) in the Chamber of Deputies—perhaps this was the "bourgeois revolution" that David Pinkney has shown did not happen in 1830. But this question goes beyond the scope of Kent's book, which is a tightly focused, splendidly documented reconstruction of a single event.

EDGAR LEON NEWMAN

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ALAIN CORBIN. *Archaisme et modernité en Limousin au XIX^e siècle, 1845-1880: La rigidité des structures économiques, sociales et mentales*. Paris: Éditions Marcel Rivière. 1975. Pp. xii, 693.

In the sixty-odd years since André Siegfried published his great study of voting patterns in western France, French thesis writers have delighted in the massive regional monograph. To the social structure and political behavior of which Siegfried constructed his analysis, scholars schooled by Simiand, Labrousse, and Braudel have added *conjoncture*: the swings and transformations of economy and population. Thus the classic model exemplified by the theses of Georges Dupeux, Paul Bois, and André Armengaud runs from a detailed geography of landholding, population, and pro-

duction, to temporal fluctuations in these and other regards over the region as a whole, to the historical geography of political behavior.

It will be hard to execute that model more faithfully, thoroughly, and competently than Alain Corbin has done for the Limousin—the departments of Corrèze, Creuse, and Haute-Vienne—during the middle decades of the nineteenth century. When it comes to the distribution of landholding, for example, Corbin analyzes the cadastres of a full sixty-eight communities covering almost a quarter of the region's total area. When it comes to popular culture, Corbin provides evidence, ordinarily built around a nucleus of statistical description, concerning literacy, school attendance, bookpeddlers, religious practice, newspapers, and theaters. There are long, rich sections on food consumption, transportation lines, agricultural practices, poverty, and voting behavior.

In comparison with work other French researchers have done on similar materials, Corbin's study contains only a rudimentary demographic analysis, little effort to apply statistical models to the data, and not much in the line of textual analysis or collective biography. But description there is, in cornucopian abundance. Unifying themes are less abundant. Migration is one; the Limousin was one of the great exporters of migrant workers to other parts of France. Corbin circles continuously around the causes, correlates, and effects of temporary migration. Among other things, he disposes of the simple notion that the great seasonal migration of construction workers to Paris and other cities provides the essential explanation of the region's rural radicalism. Instead, he offers substantial evidence that the migrant areas (and, very likely, the migrants themselves) wavered between leftism and Bonapartism, while the industrial workers of Limoges and an important segment of the region's petty bourgeoisie each developed their own reasons and their own organization for supporting radical programs by word, vote, and deed.

In this and other regards, Alain Corbin stays almost obsessively within his regional boundaries, and he rarely breaks out of his temporal limits—there are no comparisons with other regions, no comments on France in general, and nothing about the ways in which yesterday's Limousin shaped the Limousin of today. Within his boundaries, he paces off the ground with relentless, resounding steps.

CHARLES TILLY

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RULON NEPHI SMITHSON. *Augustin Thierry: Social and Political Consciousness in the Evolution of a Historical Method*. (Histoire des idées et critique littéraire,

volume 129.) Geneva: Librairie Droz. 1972. Pp. 316.

"This present study," writes Rulon Smithson, "does not attempt to systematically and generally alter judgments made of Thierry nor to arbitrate between him and his critics. Moreover, while it includes some of those judgments as illustrative of themes discussed, it is not in any sense a summation of all that has been written on the man and his works. It is, rather, an attempt to synthesize his works, philosophy, and method, and particularly to probe into their *raisons d'être*." Such is the stated purpose of this book, and that is precisely what Smithson achieves in this excellent study of the works of Augustin Thierry.

The author approaches his subject chronologically, beginning his study with background material on the life of Thierry and ending with a general conclusion. Thierry was, of course, very much the product of French culture; indeed, it would be hard to imagine any other European society producing the man that Thierry represents, especially the intellectual. He was a mild sort of rebel, in a gentlemanly sort of way, who, once obtaining acceptance by the powers that be, gradually conformed more and more to the accepted values and power structure (that is, Catholicism and even the monarchy). He aimed for the traditional rewards of his society, whether as a member of the Legion of Honor or the Académie des Inscriptions. He suffered perhaps the worst of all possible fates for a young scholar, when at the age of twenty-nine he found he was going blind.

After completing his studies at the École Normale de Paris, he bypassed teaching for research, beginning with the controversial Saint-Simon (1814-17), then graduating to the writing of mainly political articles for various papers, including the *Censeur européen* and the *Courrier français*, in which he expressed his views on liberty (a compulsive subject for young Thierry) and the need for major changes in government. At this stage he started work on his various books, beginning with his famous *L'histoire de la conquête de l'Angleterre par les Normands*, *Lettres sur l'histoire de France*, and *Récits des temps mérovingiens*.

Smithson is an old-fashioned scholar; that is to say, he loves his work and does it impeccably. Unlike so much scholarship today, Smithson's study of Thierry reflects calm, meticulous, and thoughtful work. Not only has he gone through everything Thierry ever wrote, as would be expected, but he has made careful comparisons of texts and different editions of each work, noting the changes in each and commenting on their significance.

From a physical viewpoint, this book has two minor drawbacks. The table of contents is found

on page 305, sandwiched between the concluding chapter and the bibliography, which is very awkward. There is no index. The book is admirably footnoted, however, and contains an excellent bibliography.

Augustin Thierry: Social and Political Consciousness in the Evolution of a Historical Method has immediately established itself as a work of major and long-lasting importance for anyone interested in Thierry and historical scholarship in the first half of nineteenth-century France.

ALAN SCHAM
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JAMES FRIGUGLIETTI. *Albert Mathiez: Historien révolutionnaire (1874-1932)*. Translated from the English by MARIE-FRANÇOISE PERNOT. Introduction by JACQUES GODECHOT. (Bibliothèque d'Histoire révolutionnaire, third series, number 14.) Paris: Société des Études Robespierriennes. 1974. Pp. viii, 261.

This carefully researched and well-written biography of the distinguished French historian Albert Mathiez can be read on three levels. As a case study in institutional history, it sketches the career of a member of the first generation of students trained in the methods of "scientific" historical scholarship that had been introduced in the French university system toward the end of the nineteenth century. Its treatment of Mathiez's political activities highlights the close connection between historical writing and politics in France that Mellon, Farmer, and others have examined in previous works. On the psychological level, it portrays an irascible misanthrope whose obsessions and resentments adversely affected his professional and political behavior.

By focusing on Mathiez's checkered career, Friguglietti exposes the institutional framework of the French historical profession, notably the informal system of rewards and penalties employed by the masters of the discipline to enforce professional standards. The book reveals how the son of a provincial innkeeper was able to gain entrée to the centers of academic prestige in Paris and reap the benefits of that privileged status. It then proceeds to demonstrate how Mathiez's subsequent refusal to observe the established conventions of the profession impeded his advancement and denied him his rightful place in the professional hierarchy.

The link between historical writing and contemporary politics constitutes a dominant theme of the work. Friguglietti skillfully traces the origins of Mathiez's scholarly preoccupation with religious developments during the French Revolution to the historian's own participation in the anticlerical campaign launched at the turn of the century. Later, Mathiez's mounting interest in the economic problems of France during the Revolution-

ary wars is attributed to his disillusionment with the French government's economic policies during the First World War. Such observations serve as instructive reminders that history is never written in a vacuum, even by scholars devoted to the ideal of scientific detachment.

Unfortunately, the psychological sources of Mathiez's compulsive behavior elude Friguglietti's probing analysis. One gets the general impression of a troubled individual laboring to resolve in his professional life traumas of his childhood, but the precise nature of these formative experiences remains a mystery. The famous vendetta against his mentor Alphonse Aulard, for example, cannot be accounted for merely by the two scholars' historiographical disagreements. The ferocity with which the disciple hounded the master right up to his death demands a more convincing explanation. Friguglietti's failure to supply one is due not to any defect in his scholarship but rather to the dearth of primary sources: Mathiez's papers were destroyed and his close associates were all dead before the study was begun. The author makes an admirable effort to piece together the disparate parts of the puzzle, exhausting all available sources and subjecting the meager data to an imaginative evaluation. He cautiously ventures the hypothesis that Mathiez's vituperative treatment of Aulard may have represented displaced hostility toward his hard-drinking, wife-beating father. But the psychohistorical speculation ends on a frustrating note of uncertainty, as indeed it must in the absence of sufficient documentation.

Notwithstanding the minor disappointment caused by the unanswered questions of psychological motivation, this biography represents an important contribution to the growing body of scholarship on the institutional and political context of the historical profession in France.

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DORIS S. GOLDSTEIN. *Trial of Faith: Religion and Politics in Tocqueville's Thought*. New York: Elsevier. 1975. Pp. xi, 144. \$10.00.

The subtitle of this book aptly expresses its principal theme, which the title may obscure. Carefully researched and well worth reading, this study especially examines the place of religion in Tocqueville's writings and, correlatively, in his political actions. Only to a far lesser extent does it examine religion in his personal life, although it consistently makes clear that the ambivalence of that personal "faith life" influenced his speculative and practical perception and expression of religious matters.

Doris Goldstein notes that "perhaps the chief

lesson to be derived from close examination of Tocqueville's religious outlook is the inextricable meshing in his mind of 'faith,' 'truth,' and 'utility.' " This characteristic did not make it easy for his contemporaries to judge that outlook, and it has been no easier for later interpreters. In accord with their outlooks, Tocqueville emerges as a secular liberal or a secular conservative or an ultimately convinced Catholic. Goldstein has performed a singular service in disentangling the separate strands that went into Tocqueville's thought on religion and then in uniting them again in a way which makes clear that "in Tocqueville's social and political theory both religion and political participation are held in balance as the two irreducible elements necessary to a good society."

The somewhat repetitious and prosaic style of the book is more than compensated for by care, sobriety, and nuance. It starts with the problem of Tocqueville's personal religious belief. It proceeds to his perception of religion in America as the success of an experiment, the effort at reconciliation of Church and State in the July Monarchy, and religion in the shadow of revolution. The Church during the old regime and in the Second Empire exemplifies the roots of discontent. Religion East and West are compared, and there is a summary chapter on Christianity, politics, and history.

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WILLIAM R. TUCKER. *The Fascist Ego: A Political Biography of Robert Brasillach*. Berkeley and Los Angeles: University of California Press. 1975. Pp. x, 331. \$21.50.

French fascist intellectuals, and the groups they represented, are becoming more and more the subjects of study and analysis. Nolte, Tannenbaum, Weber, Soucy, and others have made important contributions; more recently Frank Field has examined the careers of Bernanos and Drieu la Rochelle; and the January 1975 *Revue d'histoire de la deuxième guerre mondiale* was devoted to "Visages de fascistes français." To this tradition of scholarship is to be added William Tucker's detailed study of Robert Brasillach, a distinguished author, critic, and long-time contributor (later editor) of the fascist review *Je suis partout*.

Tucker devotes more than half his book to Brasillach's intellectual growth prior to the outbreak of war in 1939. In the thirties Brasillach developed an admiration for the "revolt of youth," especially as manifested in comradeship and in a joyful attitude toward life, elements he observed in German youth movements. The book is, however, a political biography; Tucker is trying to discover why a

French intellectual should have leaned so strongly toward fascism. What emerges is the portrait of a brilliant compulsive writer who, while filled with internal contradictions and in many instances with a blindness toward obvious facts, nevertheless retained a stubborn, indeed tragic, steadfastness to the convictions he acquired in his formative years. The nature of Brasillach's thought, anarchist and antimodernist on the one hand but imbued with nationalist loyalties on the other, impelled the writer to criticism of the Pétainist state and finally, completely disillusioned with Vichy, to a total reliance on Germany. By 1943, despairing of any viable unity or spirit among French fascists, Brasillach had to face the reality that the German mastery of Europe was not all joy and chivalry. He resigned the editorship of *Je suis partout* but could not deny his consistently pro-German stance. When Paris was liberated he voluntarily surrendered; he was tried as a collaborator, found guilty, and executed.

In portraying the attitudes of French intellectuals (for the book has broader coverage than the biographical format would suggest), Tucker reveals a facet of French collaboration that complements the economic and political studies of Milward and Paxton. In so many instances initiatives came from the French, who saw the German occupiers through their own self-serving lenses and who failed to see, or noted too late, that German cooperation continued only so long as it served Nazi interests. The fact that Tucker's book is not restricted exclusively to Brasillach but reveals a panorama of the French Right, provides the volume with value that the title does not explicitly reveal. As an admirable contribution to the growing literature on French fascism, this book is worth attention.

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CLAUDE BELLANGER *et al.* *Histoire générale de la presse française*. Volume 4, *De 1940 à 1958*. Paris: Presses Universitaires de France, 1975. Pp. 486.

In modern times the French press has been the drudge of the political establishment and, more recently, of big business ("les trusts") as well. This servile condition was present even before the defeat of 1940. The Nazis knew that a captive press is not for the expression of public opinion but for its conditioning. Under the *Propaganda-Abteilung* and the German Embassy in Paris, the French press in the northern zone became entirely dependent upon the occupier. The Germans, however, were not satisfied with the vociferous support of collaboration by this press, whose chief magnate was Jean

Luchaire ("louche Herr" in Galtier-Bossière's phrase) and that boasted such anti-Semitic, anti-communist, and anti-Masonic rags as *Au Pilon*. A *Propaganda-Abteilung* agent named Hibbelen acquired direct control over forty to fifty percent of Parisian publications by May 31, 1944. Ambassador Otto Abetz, also extremely active, distributed a billion francs in subsidies to such newspapers outside Hibbelen's empire as Maurice Bunau-Varilla's *Le Matin*, Marcel Déat's *L'Oeuvre*, and others.

Vichy, too, was a strict master of the southern press long before the German "supervisors" arrived with total occupation. The *Secrétariat Général à l'Information*, directly subordinated to Pierre Laval and headed by such zealots as Paul Marion and the charismatic Philippe Henriot, exercised effective censorship by means of *notes d'orientation* and prohibitions (*consignes*). The subsidized Vichy press displayed the same servility as its occupied partner to the themes of anticommunism, anti-Semitism, and collaboration, to which were added three new passwords: *Révolution nationale*, anti-Gaullism, and anti-"terrorism." After November 10, 1942, an occasional smuggled Swiss newspaper and the British radio seemed the only sources of unbiased news available to the French public.

This gap was soon bridged by the clandestine press. Beginning with a pamphlet printed in August 1940, the "press of the shadows" grew after 1942 to include 1,106 publications of the Resistance organizations themselves and the former political parties. A clandestine news agency was in liaison with London, which supplied the necessary funds. Always anticollaborationist and, after November 1942, anti-Pétainist, the personnel of the Resistance press yearned for a drastically reformed post-war republic, for social justice, and for international peace. In March 1944 it created a national body to draw up plans for the seizure of the collaborationist presses and plant when liberation arrived.

The great moment in Paris came on August 21, 1944, when eleven Resistance newspapers occupied the buildings of the Vichy and pro-German press, left vacant by their owners two days before. In the "distribution of the spoils," as former Vichyites and former collaborationists would term it, the Communists got the major share, followed by the Socialists, and finally by the Christian Democrats. These confiscations were legalized by the law of May 11, 1946, which created an agency to oversee the liquidation of 482 publications. Meanwhile, a purge swept through the ranks of the *ci-devants*: some, including Luchaire, were executed; others were either given long prison sentences or denied their press identity cards.

But the morning after the euphoria of liberation was bleak indeed. Too many dailies, inflationary

costs, bad management, and crippling strikes dealt a terrible blow to the postwar press. By 1958 the number of Parisian dailies had fallen from 29 to 12, of provincial dailies from 161 to 110. Certainly there were some brilliant successes, such as Pierre Lazareff's *France-Soir*. The press had a majority in the governing board of the central news agency, France-Presse; censorship and prior authorization to print (*l'autorisation préalable*) were ended. But Hachette still retained its strangle hold over the distribution of printed material. The state continued to control the main agent of advertising, Havas-Publicité. Above all, a general statute concerning the press had not been passed by the harried assemblies of the Fourth Republic by the time of the regime's demise in May-June 1958.

This volume brings together widely scattered and out-of-print materials. As a synthesis, it is especially successful in the first section by Henri Michel and Claude Lévy. The second and last sections are comparatively less objective, owing to the fact that the author, Claude Bellanger, was an important press personage during the period covered. A highly interesting, if intricate and prolix, chapter by Fernand Terrou concerns the evolution of French law dealing with the rights of the press. Close attention is paid to provincial publications. There are informative tables, graphs and charts, and two indexes, one of individuals, the other of press titles. The illustrations are well chosen: the last one shows a miniskirted girl reading *Le Monde* on a restaurant banquette, as a Parisian matron, holding *Le Figaro*, gives her a sharp glance of disapproval.

There are errors. Pierre Dominique is correctly identified as the director of Vichy's news agency, but his postwar editorial work in the most important pro-collaborationist weekly, *Rivarol*, is not mentioned. Joseph Rossé did sell his *Elsässer Kurier* to the Germans in 1941, but the fact that his publishing house, Alsatia, became the major source of illegal religious books for the wartime Reich is omitted.

Despite these and other inaccuracies, this volume is an invaluable resource for the historian working in the field of recent French history. It is not likely to be surpassed for some time to come.

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STANLEY G. PAYNE. *Basque Nationalism*. (The Basque Series.) Reno: University of Nevada Press. 1975. Pp. xii, 291. \$9.50.

Stanley Payne finds the genesis of a "sense of identity" within the Basque territories in the tenth century, indicates that this Basque nationalism

reached its apogee after the fall of the Spanish monarchy in 1931, and confirms that a strong sense of nationalism continues to this day. Discounting discussions of a Basque "race" as scientifically unsound, he attributes the Basque sense of ethnic identity to language, culture, and economics, and he emphasizes that these bonds are, paradoxically, also barriers to unity since there are several tribal dialects of *Euserka* that are mutually unintelligible and since the four Basque provinces have traditionally been divided socially, politically, and economically between the two western, industrialized and progressive provinces in contrast to the two eastern agricultural provinces. A primary reason for unity during the one-thousand-year history expertly documented by Payne has been the attempt to maintain the *fueros*, the local rights and privileges (especially favorable tax privileges) granted to the Basque territories as early as the eleventh century and swept away finally by Franco eight hundred years later.

In *Basque Nationalism* Payne offers not only a well-documented history of the Basque territories, but he also provides a clear view of the Catalanian independence movement, a movement that led the way for the Basque quest for autonomy. Of special interest in the book is the Basque perspective of the Spanish Civil War, a war that the author also describes as a civil war between the Basques of the four provinces.

A detailed map of Spain and perhaps of the four "separatist" regions described (Galicia, Valencia, Catalonia, and Las Vascongadas) would make the volume more accessible to the reader not totally familiar with the peninsula; there is only one map of the Basque provinces with a small inset of the Iberian Peninsula. A selection of photographs illustrates the present-day Basque nationalist movement. Charts abound throughout, describing political inertia, vote counts, book publication, percentage of wealth, blood types, and political crimes. The author's clear and straightforward prose style, his insight into the nature of the Basques and of the Basque territories, and the thoroughness of his research will excite the Hispanist as well as the student of ethnic enclaves. *Basque Nationalism* is a cornerstone of Basque historiography that will serve the historian in this post-Franco era.

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GEOFFREY PARKER. *The Army of Flanders and the Spanish Road, 1567-1659: The Logistics of Spanish Victory and Defeat in the Low Countries' Wars*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1972. Pp. xviii, 309. \$23.50.

This book goes well beyond military history by providing information that should prove valuable to political scientists, sociologists, and psychologists interested in Spain, France, Germany, and the Netherlands during the Eighty Years' War.

Geoffrey Parker uses Spanish, Dutch, French, and English documents with commendable skill, and he includes statistical illustrations and maps that are imaginatively conceived and invariably helpful. Spending just the right amount of time on technical matters, he gives a comprehensive picture of the Spanish army and of warfare during the sixteenth and seventeenth centuries. The reader gets a close look at Spain's great war machine (prone to sudden and disastrous breakdowns) and at the faces of the soldiers themselves—the noblemen, adventurers, and conscripts—marching from Italy along the Spanish road, over the Alpine passes, and into the Low Countries, where they quickly became food for powder or fell prey to unscrupulous captains and creditors.

There is a wealth of detail about the long march of over seven hundred miles. Parker furnishes several maps that reconstruct the main routes, and he gives a full account of what kept the army going: *étapes* (provisioning centers), road-clearing and bridge-building, food distribution, baggage transport, and financial arrangements with local magistrates and merchants. He discusses, too, the devastating effects of the army on friends and foe alike. "The moving army could become a vast incubator of plague," and inevitably it strained the agricultural resources of every town along the way.

No work that I have read offers a comparable account of life in the Spanish army and of the bureaucracy that loomed behind it. Parker's explanation of Spanish advances, notably in 1583–87, is thoroughly convincing. Equally perceptive is his analysis of subsequent failures—the result of political miscalculation; dwindling resources; corruption; mutinies (which "became virtually an institution of military life and form one of the earlier chapters in the history of collective bargaining in Europe"); the necessity of waging war on two fronts; and, above all, the resurgent power of France. Of special merit is Parker's analysis of Philip II's war policy, which was doomed from the moment the Dutch first realized that they could hold out and win if they had the will to do it.

Every chapter in *The Army of Flanders and the Spanish Road, 1567–1659* should be read appreciatively. Parker has given us a book that is necessary, readable, and definitive.

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MARTIN BLINKHORN. *Carlism and Crisis in Spain, 1931–1939*. New York: Cambridge University Press. 1975. Pp. xii, 394.

Carlism, as the most enduring grass-roots right-wing movement in modern Europe, has long fascinated historians. From the mid-1820s to 1936 it offered an ultramontane, "throne and altar" alternative to the sporadically liberal Bourbon monarchy and to the shortlived republics of 1873 and 1931–36. Its popular support lay among the independent peasants and the small-town artisans and middle class primarily of Navarre, and secondarily of the agriculturally poorer portions of Aragon, Catalonia, and the Levant. In three civil wars between 1833 and 1876 it had been defeated. After 1876 the Communion was weakened by doctrinal struggles, and frequently it lacked a credible candidate for the throne. Nevertheless the Carlists played a significant role in the politics of the Second Republic and in the Civil War of 1936–39, which for them was "the fourth Carlist War."

In 1931 the Carlists moved from uneasy alliance to bitter hostility in their relations with the neighboring Basque nationalists. They dominated local government in Navarre and Alava under the Republic, and they were able during the Civil War to share power to a degree with the military authorities. While the left in 1934 was convinced that the entry of Gil Robles and the CEDA into the government would lead quickly to fascism, the Carlists were afraid that such participation would stabilize the hated Republic. At all times they asserted that the true purpose of the Republic was to introduce Marxist revolution and turn Spain over "to the lackeys of Moscow and of international Jewry." Although much of their membership came from the unprivileged classes, their deputies consistently voted against even the mildest land reform laws. During the war all but a few partisans of Fal Conde and the little-known, absent pretender Don Javier accepted Franco's imposed one-party framework.

Martin Blinkhorn has made thorough use of the *Archivo Histórico Nacional*, the Carlist press, government documents and bulletins, and biographies and autobiographies of numerous participants. The practical politics of Carlism and the leadership struggles are clearly expounded and interpreted. There is one minor weakness. The author repeatedly states that Carlism included a vital undercurrent of radical-populist protest. Except for quotations from ephemeral student publications, however, he does not offer even rhetorical, let alone practical, evidence for a social protest element before the 1960s, after which there have indeed been Marxist-Leninist Carlists and occasional Carlist members of the illegal Workers' Commissions.

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JOHN F. COVERDALE. *Italian Intervention in the Spanish Civil War*. Princeton: Princeton University Press. 1976. Pp. xxi, 455. \$18.50.

Based in large part on hitherto unexplored archival materials in Italy, Spain, Britain, and the United States, this book is the first thorough and systematic study of the history of Italian intervention in the Spanish Civil War. After tracing the background of events from 1922 to 1936 that led to Mussolini's decision to support Franco, the author provides a detailed examination of Italy's military contribution to the victory of the Spanish Nationalists, together with a parallel account of Italian diplomatic and political relations during the Civil War with Nationalist Spain, Germany, Britain, and France.

John Coverdale's basic contention is that strategic calculations played a much more decisive part in Mussolini's decision to intervene in Spain than the various ideological justifications that were trumpeted to the world by the propaganda agencies of the Fascist government. He shows, however, that despite the overriding importance for Mussolini of "traditional foreign policy considerations," there were "prestige factors" and a sense of common cause with the militantly anti-communist and counterrevolutionary posture of the Spanish Nationalists that also influenced the Italian dictator and his followers to a considerable degree.

Two fundamental conclusions that emerge from Coverdale's book are, first, that "Italian diplomatic and military backing was a crucial element in Franco's victory," and, second, that throughout the Civil War General Franco succeeded in maintaining his autonomy and freedom of action despite the heavy pressures exerted on him by his German and Italian supporters. Both of these conclusions are buttressed by abundant documentation and by a careful analysis of the available evidence.

Coverdale's rigorously objective and dispassionate attitude toward his material has enabled him to offer a balanced account of Italian intervention in the Spanish Civil War. But even emotion-free detachment has its pitfalls. Occasionally, in his effort to avoid the risks of bias in any direction, the author tends unwittingly to present as verified fact what is in actuality a highly dubious and partisan interpretation of a particular event or motive. One of several possible examples of this tendency is the statement on page 60 that, in view of the disorder and violence which characterized Spanish political life between February and July 1936, "it is not surprising that plans soon began to be laid for a military coup to restore order," as if the restoration of order, and not the

establishment of what Coverdale himself (adopting the terminology of Juan Linz) later calls "a bureaucratic-military authoritarian regime," was the primary motive behind the July 1936 insurgency against the Spanish Republic.

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KLAUS-JÖRG RUHL. *Spanien im Zweiten Weltkrieg: Franco, die Falange und das "Dritte Reich."* (Historische Perspektiven, number 2.) Hamburg: Hoffmann und Campe. 1975. Pp. 414. DM 58.

The foreign relations of the Franco regime have become the object of considerable study during the past twenty years, but most of this work lies hidden from view in unpublished dissertations. The content of Klaus-Jörg Ruhl's book—itsself based on a former dissertation—is indicated by the subtitle. This is not exactly a history of "Spain in the Second World War," or during the war, for the real study of Spanish society in that period of privation and suffering is still largely unexplored. Rather, it deals primarily with German-Spanish relations, with the general policy of the Spanish state during the war years, and with crucial maneuvers within the circle of Spanish Nationalist politics directed either toward provoking or avoiding Spanish entry into the war. The history of the Spanish Blue Division on the Russian front, by now the subject of a massive bibliography all its own, has prudently been for the most part excluded.

This is clearly the best and most comprehensive study that has yet appeared of Spanish-German relations and their influence on Spanish government and policy. The previous books by Donald Detwiler and Charles Burdick dealt with more limited or specialized aspects, while the recent work by Raymond Proctor centered in large measure on the activities of the Blue Division. Ruhl's contribution consists not merely in achieving a more comprehensive scope but in a more extensive and thorough exploitation of German and English materials, both published and unpublished, than has previously been made. If the result does not produce many new surprises on the level of official diplomatic history, some very interesting new data are offered on the machinations of NSDAP representatives, certain other German officials vis-à-vis dissident radical elements of the Falange, and, by 1942, General Muñoz Grandes (first commander of the Blue Division), to induce a radical change in the course of the Spanish government, a falangization of the regime, and the possible ouster of Franco himself. The nearest thing to a hero produced by this story is probably Eberhard von

Stohrer, the German ambassador from 1937 to 1942 who sought to avoid direct German interference in internal Spanish politics and thereby limited the possibility of Spanish intervention in the war.

In the process, Ruhl provides a slightly different perspective from that frequently encountered in commentaries on the Franco regime. He does not stress as heavily as others the astuteness of Franco; while recognizing the latter's undeniable capacity for dissimulation, temporization, and maneuver, Ruhl tends to place more emphasis on the influence of objective external factors and limitations that were beyond Franco's power to manipulate. Though he does not propound any general theory or interpretation of fascism, he also avoids the danger of reading the post-1945 doctrines and structure of Franquism too far back into the regime's founding decade, when certain parallels with other authoritarian regimes in Europe were more pronounced and seemed capable of further development. In general, this work stands as a significant contribution to the understanding of Spanish-German politics during the war years.

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HERVÉ HASQUIN. *Une mutation: Le "Pays de Charleroi" aux XVII^e et XVIII^e siècles. Aux origines de la Révolution industrielle en Belgique.* (Études d'histoire politique, économique et sociale.) Brussels: Éditions de l'Institut de Sociologie, Université Libre de Bruxelles. 1971. Pp. 383. 498 fr. B.

Charleroi, the great Belgian industrial city, started in 1660 as a fortress overlooking the river Sambre and was named after young king Charles II of Spain. By 1800 it had become the locus of incipient coal, steel, glass, nail, and textile industries. In the first part of his book, Hervé Hasquin relates government policies and the story of private entrepreneurs to the early growth of this industrial center. He narrates the construction of roads to Brussels and to the ancient mining areas of the surroundings and the granting of royal privileges and other measures favoring industrial development, such as the implantation of fairs and markets and the grant of liberal import and export duties.

The role and achievement of a few successful entrepreneurs is described with a great wealth of detail. This part of the book is naturally geared to legislative history, to the exhaustive compilation, study, and exploitation of charters, resolutions, protocols, ordinances, correspondence, inquiries, and notarial records. The next part of the book is an attempt to locate turning points in the growth of particular industries and of aggregate economic activity, and to relate demographic trends to the

record of economic growth. This quantitative study and the establishment of a time series of production and trade lead to the observation that the period 1763-74 was one of rapid growth in the level of economic activity and to the conclusion that economic growth led to rapid population growth by way of immigration and high birth rates.

On the one hand this book is disappointing. Although the author attempts to convey his enthusiasm about the subject by way of a very strong, sometimes bombastic, vocabulary, his work is essentially a narrative lacking a thesis and an analytical backbone. The book cannot be characterized as innovative in its method or in its use of sources. Yet the author can be commended. He is remarkably familiar with the Belgian literature for both parts of the country in both French and Dutch—a not so frequent occurrence in Belgium—and often brings in references to the economic history of France and even England during this period. Above all, Hasquin should be congratulated for having written a thoroughly researched and documented narrative work of local history to be used profitably for reference and comparison by other historians.

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B. A. SIJES. *Studies over Jodenvervolgung.* Assen: Van Gorcum. 1974. Pp. viii, 182.

This volume is a collection of studies prepared in connection with the work of the Rijksinstituut voor Oorlogsdocumentatie (Netherlands State Institute for War Documentation) with which the author has been connected since its establishment in 1945. The individual papers were prepared for a variety of purposes including postwar trials (such as the paper on Eichmann's role in the Netherlands). The collection covers a wide variety of topics within the general theme of the Holocaust.

The first paper, "The Ostmark as Testing Ground," deals with certain aspects of anti-Semitism in Austria since the nineteenth century. Sijes points out that anti-Semitism proposing extreme solutions for the "Jewish question" was rife in Austria at least since the middle of the century. In the final section of the article the author describes the methods used after the *Anschluss* to force quick emigration of Jews.

The second selection, covering emigration from Germany, deals with Nazi measures used to accelerate the rate of emigration of German Jews after the *Kristallnacht*. Of more general interest is the third chapter, "The Final Solution of the Jewish Question," which explores the timing of the deci-

sion to carry out total extermination. Sijes believes that the basic notion to exterminate the Jews was in Hitler's mind as early as the 1920s, but it took concrete shape in 1938-39. Sijes considers the killings by the SS during and after the Polish campaign as one more indication of Hitler's long-range plan. The comprehensive extermination by gas is believed to have been decided by March 1941. Sijes doubts that the German leaders ever considered the Madagascar plan a serious alternative, certainly not after the outbreak of the war.

The fourth paper deals with the activities of the SS officer, Erich Rajakowitsch, who had been one of Eichmann's deputies in Vienna since 1938. Rajakowitsch is shown to have played an active role in the deportation and to have taken extreme and harsh positions with regard to exemptions and the speed of deportations. Chapter 5 deals with Eichmann's personal and direct contribution to the extermination of Dutch Jews. Eichmann is shown to have urged a radical and accelerated effort on the part of the Germans to deport all Jews, including persons who, within existing regulations, could be exempted from "resettlement" (Jewish husbands of Aryan partners, etc.). Sijes makes the interesting point that Eichmann's low position in the organizational table of the German police and SS and his low rank are no indication of the importance of his role. In the Netherlands Eichmann dealt directly with the highest German officials such as Reichskommissar Seyss-Inquart on questions of policy, while directing lower police officials on specific questions dealing with the implementation of such policies. Sijes points out that Seyss-Inquart insisted on keeping ultimate control over the measures against the Jews, but that the police in effect managed to eliminate to a considerable extent interference from lower-echelon representatives of the civilian administration as deportations got underway. For all practical purposes, it was Eichmann who from Berlin controlled the rate at which Dutch Jews were deported after June 1942.

Chapter 6 contains some general observations about the position of the Dutch Jews during the occupation. Sijes' assessment of the role of the Secretaries-General (the highest Dutch government officials left in the country) and of the Amsterdam Jewish Council is harsher and contains fewer qualifications than do other treatments of the same issues (de Jong, Presser, Herzberg). On the question of why so many Dutch Jews perished (eighty percent—the highest percentage of any occupied country in the West), he makes a judicious and unemotional assessment, ending up with the conclusion that the Dutch Jews reacted to the German threat as most other groups in Dutch society did or would have done under similar cir-

cumstances. While recognizing anti-Semitic tendencies among the Dutch people, he pays tribute to the many non-Jewish Dutchmen who in helping the Jews go into hiding risked and often lost their own lives. The seventh chapter, an appreciation of Simon Wiesenthal and his work, is the text of a talk given by Sijes on the occasion of the twenty-fifth anniversary of the inception of Dr. Wiesenthal's internationally known work in locating war criminals.

The final chapter, the text of a lecture given in 1969, deals with the then current issue whether three German police officials, the last Germans held in Dutch prisons, should be released before the end of their term. Sijes takes the position that these officials indeed carried major responsibilities for the destruction of Dutch Jewry, and that the purposes of justice would not be served by a premature release.

In summary, this collection of papers, while somewhat uneven, contains many new facts and insights which will be of use primarily to the specialist dealing with the Holocaust and the German occupation of the Netherlands during the Second World War.

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ELOISE ENGLE and LAURI PAANANEN. *The Winter War: The Russo-Finnish Conflict, 1939-40*. New York: Charles Scribner's Sons. 1973. Pp. xv, 176. \$7.95.

Few modern wars have received the kind of awed global attention accorded to the 105-day-long Russo-Finnish Winter War of 1939-40. It seemed for a while to be a case of David defeating Goliath. Some 450,000 Soviet troops supported by 1,000 tanks and 1,000 planes embarked on what Moscow expected to be an easy two-week excursion through Finland, only to be spectacularly routed by 215,000 Finns (including reserves) who had no tanks and only 75 combat aircraft. Regrouping for a second effort, the Russians advanced with no less than forty-six divisions, more planes, and an abundance of artillery, and by March 13 the Winter War was over. The gallant Finns counted 80,000 casualties, including 22,900 dead and 10,000 permanently disabled, while the Russians admitted a loss of 217,500 dead. Thirty years later Khrushchev raised that figure to 1,000,000.

The military history of the Winter War has appeared before in many languages, and in English most recently by Richard W. Condon in *The Winter War* (1972) and Allen F. Chew in *The White Death* (1971). Eloise Engle's and Lauri Paananen's book adds nothing to those studies; indeed, it falls far short of their quality. Only the battles of Tolva-järvi and Suomussalmi and the Finns' *motti* tactics

are reasonably well handled. The perfunctory historical introduction is a mishmash of tired clichés, uncritical tributes, and misinformation, while the book's source base is utterly insufficient for a serious effort. Typically, the bibliography omits the Chew and Condon books along with some basic Finnish works, while it includes the Finnish-language editions of some works available in English (for example, Douglas Clark's *Three Days To Catastrophe*). In sum, this book has nothing to offer serious students of the Winter War.

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ALAN MAYHEW. *Rural Settlement and Farming in Germany*. New York: Barnes and Noble. 1973. Pp. 224. \$14.00.

The literature of German rural settlement history seems a choppy sea of diffuse local studies from which arise some misty islands of inconclusive controversy. The working descriptive vocabulary is specialized but at the same time imprecise; definitions of settlement patterns and tenure arrangements are likely to disintegrate when applied to concrete situations. Ethnocultural issues that gave the field its form are outdated. This situation allows ample room for more special studies and for theoretical speculation, but anyone seeking a general understanding of German rural settlement history is confronted by a morass of esoterica, qualifications, and exceptions. The signposts we need are missing or obsolete.

Alan Mayhew's short summary cannot solve the scholarly problems of the field. He offers no new general synthesis, but he performs the very important service of providing his readers with some basic equipment for coping with materials on German rural history. His narrative begins with the earliest known settlement forms, a description that must be anecdotal and localized because close research has discredited the sweeping patterns postulated by earlier nationalist scholarship. His chapter on medieval settlement offers a taxonomy of settlement forms, whose patterns result from the planning roles of ecclesiastical and noble entrepreneurs and their professional *locatores* or developers, not the ecological or cultural circumstances of peasant settlers. Mayhew accepts Abel's "late medieval agricultural depression," perhaps without presenting enough detail or analysis to make that characterization satisfactory. He locates a clear separation between East and West Elbian patterns in the early modern period, ascribing this also to diverging forms of political authority. In a final chapter on enclosure and consolidation he traces modern developments: toward the family farm in

the West, confirmed by the *Flurbereinigung* in the Federal Republic and the *Gutsbetrieb* in the East, from Junker to collective.

If one seeks the peasantry itself through these descriptions of their settlements, the picture that emerges is one of lonely and passive families without real autonomy or mutual association; they are manipulated by the powerful, and by accident; their affective relations are with physical property and with force beyond their influence; and if they have cultural identity it leaves little mark. This impression surely results in part from the nature of the evidence, but it is enough to suggest that the peasantry may be the last place to look for the kind of intrinsic anthropological or sociological structure that scholars have traditionally sought among them most earnestly; it suggests again the nagging question of whether a category of people without an autonomous and conscious history of their own is amenable to historical description.

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DIETER SCHWAB. *Die "Selbstverwaltungs-idee" des Freiherrn vom Stein und ihre geistigen Grundlagen: Zugleich ein Beitrag zur Geschichte der politischen Ethik im 18. Jahrhundert*. (Giessner Beiträge zur Rechtswissenschaft, number 3.) [Frankfurt:] Athenäum Verlag. 1971. Pp. 164. DM 36.

For a statesman who occupied high office only a very short time and who is remembered more for what he wanted to do than for what he did, the Freiherr vom Stein has received a remarkable amount of attention from historians, and not only from those of his own country. In part, at least, the sheer quantity of this writing reflects the liveliness of the debate that raged for years concerning the interpretation of Stein's ideas for Prussian reform. A decade ago, however, in a judicious survey, "Stein in German Historiography" (in vol. 5, *History and Theory*), Klaus Epstein concluded that most of the points once in dispute could now be considered as "definitely settled." In particular, Epstein noted that "there is now general agreement on the origins of Stein's ideas." Max Lehmann's thesis of Stein as the liberal disciple of the ideas of 1789 no longer had adherents, whereas post-Lehmann scholarship, whose crowning achievement was Gerhard Ritter's biography, had established that Stein's ideas, in so far as they derived from intellectual "influences" at all, linked up with Möser, Herder, Montesquieu, and Stein's friends of the Anglo-Hanoverian circle, Brandes and Rehberg.

This interpretation is now challenged by Dieter Schwab. Taking as his point of departure Stein's

"idea of self-government" (actually a complex of ideas that, it is generally agreed, pose a central problem for the understanding of Stein), Schwab concludes that the reformer's true intellectual home is found among the English and Scottish moral philosophers: Shaftesbury, Hutcheson, Lord Kames, Adam Ferguson especially, Paley, and their German disciple, Christian Garve. Not one of these names is found in the index of Ritter's biography. Schwab bases his conclusions on a study of Stein's library and a close analysis of his writings. He has produced a tidy, original, and persuasive book.

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RAINER POSTEL, *Johann Martin Lappenberg: Ein Beitrag zur Geschichte der Geschichtswissenschaft im 19. Jahrhundert*. (Historische Studien, number 423.) Lübeck: Matthiesen Verlag, 1972. Pp. 352. DM 54.

Johann Martin Lappenberg (1794–1865) is a historian about whom relatively little has been written either in Germany or elsewhere. After reading Rainer Postel's study, it is easy to see why. This is not meant to denigrate the author's work, which is a well-written, painstakingly researched study of a historian who was rather well known during his lifetime. Lappenberg's contacts with such better-known historians as Ranke, Niebuhr, and Dahlmann are described with convincing thoroughness. Nevertheless, as Postel himself points out, Lappenberg was not well known as an author of major historical works; basically, he was a major contributor to the establishment of *Quellenkunde*, that tedious but necessary aspect of the historical discipline quite often (and inaccurately) identified as an achievement of Ranke.

Lappenberg, born and raised in Hamburg, was primarily concerned with gathering, collating, and editing sources, especially those concerning the history of northern Germany. In this regard he was also interested in editing and bringing up to date major secondary works, such as Georg Friedrich Sartorius's documentary history on the origins of the Hansa. Lappenberg also tried his hand at literary history and here was primarily concerned with describing the historical contexts within which literary figures wrote and from which they drew models for plots and characters. It is perhaps ironic that the only major historical work which Lappenberg actually published was a history of England, a country for which he had a great deal of admiration. Thus, as a practicing historian, Lappenberg's contributions were minimal. In truth, he was more of an archivist.

What makes Postel's work of interest is how his subject reflected major concerns and prejudices of

his time. Politically conservative, Lappenberg was concerned that the apparently moribund Hamburg Senate, as well as the society it represented, not be swept up in putatively democratic currents, such as those revealed in 1848. At the same time, though, he obviously was very much influenced by the upsurge of cultural nationalism that was occurring throughout Germany during the post-Napoleonic and *Vormärz* periods. This came out most clearly in his history of England, where he favored the early Britons and the Anglo-Saxons over Roman and Norman conquerors. Like Father Jahn, he chose to view the English as being Germanic in character, possessed of a sort of honesty and robustness that set them apart from those from southern climes.

Furthermore, Postel succeeds in pointing out the general nature of the move toward historicism. In this work, it emerges as being a sort of fusion of Romantic views toward history and a more rigorous philological-critical method. While Lappenberg may not have written many original historical works, he was definitely a part of a historicist trend, as reflected in his own concern that those who write history do so with a scrupulous concern for capturing a past epoch as it actually was. He also expressed another belief central to the historicist method, namely, that the historian, being *zeitgebunden*, is writing history for his own time even though he might well be attempting to capture the past *wie es eigentlich gewesen ist*. What Postel establishes is that historicism was a general trend in Germany during the *Vormärz* period and further, that historicism and political conservatism went together. Also, Postel's treatment of Lappenberg suggests the need to qualify Ranke's position as a sort of "founder" of modern *Quellenkunde* and of modern historicism.

Postel's work does little to further illuminate Johann Martin Lappenberg as an important creative historian. Rather, the value of the work is that it reveals just how widespread were certain fundamental attitudes and trends in German historiography during the first half of the nineteenth century. In Lappenberg, we can see an unsynthesized amalgam of concerns; it required seminal figures such as Droysen, Sybel, and Meinecke to provide the required synthesis. This achievement would have results for German historiography that were both awesome and tragic.

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KLAUS J. BADE, *Friedrich Fabri und der Imperialismus in der Bismarckzeit: Revolution—Depression—Expansion*. (Beiträge zur Kolonial- und Überseegeschichte,

volume 13.) Freiburg: Atlantis. 1975. Pp. 579. DM 42.

Friedrich Fabri belonged to that remarkable group of secular evangelists who during the 1870s and 1880s helped convert Europe to the new gospel of colonialism. Some of them were explorers who returned from distant lands with stirring tales of the wealth and glory to be won beyond the ocean. Others were politicians seeking to gain or retain power on a platform of overseas expansion. Still others were jingo publicists who taught a doctrine of national greatness built on the conquest of dark races in far-off continents. Fabri himself was a member of the theological contingent of imperialism. A clergyman and the son of a clergyman, he started his public career as the administrator of a missionary society in the Rhineland. Disturbed by what appeared to him to be the spread of materialism among the lower classes, he began to see the specter of class conflict looming over the horizon. But might not the diversion of national energies to the task of colonial expansion alleviate this domestic discontent? His *Bedarf Deutschland der Colonien?*, like Seeley's *Expansion of England* and Leroy-Beaulieu's *De la colonisation chez les peuples modernes*, helped prepare the way for the last feverish crusade of the white man to subjugate the world.

Fabri has now found a serious scholarly biographer in Klaus J. Bade, who deals in minute detail with Fabri's life from the early days in the mission house in Barmen to the period of national prominence when he was one of the leaders of the colonial movement. The author, accepting the recent thesis of Hans-Ulrich Wehler, believes that German imperialism was the product of internal tensions which state and society sought to externalize through the acquisition of overseas possessions. The depression of the 1870s and the growth of radicalism persuaded the ruling classes that only success abroad could divert public attention from mounting discord at home. Fabri, of course, like most ideologues of expansionism, never admitted to others or to himself that he was merely trying to palliate the domestic crisis of a nation torn between authoritarian traditions and democratic aspirations. To him the establishment of colonies was motivated by the same selfless concern for the welfare of man as his own early interest in the poor and exploited textile workers of the Wupper Valley. The objective of imperialism, he argued, should be the "education of the native for labor," the improvement of his material condition, and the elevation of his spiritual outlook. Without realizing it, he provided a mantle of idealism for a policy of ruthless oppression.

Bade pursues the career of his protagonist through all its winding byways. Although from

time to time he links Fabri to the broad currents of German political and social life, his emphasis rests largely on the man and his activities. The scholarship is beyond reproach, at least in the technical sense; how can anyone quarrel with a work in which some 150 pages of notes and 40 of bibliography buttress a text of 360 pages? The research that has gone into the making of this book—unpublished sources, newspapers, journals, minutes and countless secondary works—is prodigious. But does Fabri deserve all this labor of love? Perhaps it is unchivalrous even to ask the question. Those who are very much interested in his life can now turn to a biography that will satisfy the most voracious appetite. Those, on the other hand, who care less about him than about the movement he represented will find occasional insight and illumination, even if they do not follow in all its intricacies the career of the Rhenish clergyman who became an apostle of imperialism.

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CONSTANTIN FRANTZ. *Briefe*. Edited by UDO SAUTTER and HANS ELMAR ONNAU. Wiesbaden: Franz Steiner Verlag GmbH. 1974. Pp. xii, 180. DM 32.

Constantin Frantz is one of those twilight figures of history known not for what they did, but for what they opposed. Frantz's target was Bismarck's Prussianized German state that he wished to replace with a German-led Central European federation extending from the Low Countries and Switzerland to Poland and the Balkan Peninsula. This scheme, derived from a romantic medievalism and a misreading of more recent history, was ignored in his lifetime, but it received some attention after 1945. Under the impact of the Nazi catastrophe it was felt that Frantz's proposed federation might have offered a more palatable alternative to Bismarck's centralized German state. On closer examination, however, his views were quickly dismissed again as dangerous and impractical.

The letters collected in this small volume contain little new information on these plans, but they are of some psychological and sociological interest. These are the *cris de cœur* of a lonely, frustrated individual, unable to hold any regular job and barely supporting himself and his family by an endless torrent of articles, pamphlets, and other tracts. Ever in need of encouragement and new sources of income, he kept ingratiating himself with the influential and affluent: Bismarck prior to his minister-presidency; Georg von Cotta, the publisher; and Richard Wagner, to mention the best known. He peddled his writings to the few sympathetic editors who would read them.

Gradually, plagued by ill-health, nonrecognition, and financial worries, Frantz found a refuge in anti-Semitism, anti-Catholicism, and other "anti" positions, and he joined the ranks of such like-minded apostles of gloom and doom as Paul de Lagarde, Julius Langbehn, and Ludwig Schemann. After Wagner started publishing his *Bayreuther Blätter*, Frantz had a ready outlet for his tirades.

Udo Sautter has written a useful introduction and has provided helpful annotations to the letters. One might question, however, the need for the vague and often redundant cross references.

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ROGER CHICKERING. *Imperial Germany and a World Without War: The Peace Movement and German Society, 1892-1914*. Princeton: Princeton University Press, 1976. Pp. xiv, 487. Cloth \$27.50, paper \$10.50.

Roger Chickering's study of the German peace movement is a "traditional" monograph on a hitherto neglected aspect of Wilhelminian society. It does not belong to the Wiesbaden Peace and Conflict Research genre, led by D. Senghaas, K.-J. Gantzel, and G. Kress. The author depicts the German Peace Society (founded in 1892) as a non-rural, middle- to lower-middle-class movement dominated by persons from the world of commerce, the lower educational system, professional organizations, and the lower municipal bureaucracies. The constituency of the society was more elderly than young, with almost thirty percent females and few Jews. By 1914 there were nearly one hundred regional peace groups with a membership of 10,000—of whom two to three hundred were active. Over one-half of these groups were in Württemberg; a mere nine were east of the Elbe River. A second peace group, the Union of International Cooperation, was founded in 1911, and three years later numbered three hundred, including many prominent historians and economists. George Hallgarten described it as a "brilliant gathering of officers without an army."

What few antecedents the German peace movement could claim stemmed mainly from the Enlightenment and romanticism. When peace societies were established in the 1880s, it was often with the aid of foreigners (England's Hodgson Pratt and Denmark's Frederik Bajer), as well as foreign financial backing (the Carnegie Endowment).

A "smug elitism" permeated the German peace movement. "The devoted pacifist was a militant crusader, convinced of his own righteousness and intolerant of criticism." Leaders such as Foerste, Fried, and Quidde sought a "facile transition from

Hobbesian assumptions to Kantian conclusions" through educating the masses, simultaneously limiting armaments of all nations, and creating an international political organization to regulate conflicts among the powers. They generally failed to influence the press, the educational system, political parties, the imperial government, and the churches (by 1912, only 117 of 35,000 Protestant and Catholic clergymen belonged to the Peace Society). Nor did they find converts among Masons, business leaders, or university professors. The "progressive bourgeois left," united in 1910 in the Progressive People's party, alone supported the German Peace Society. And although politically liberal and democratic, the movement's members were socially conservative, and hence they had little influence upon Social Democratic policies until 1911-13. The peace groups also failed to attract members beyond left liberals and socialists (Bismarck's *Reichsfeinde*) because they championed a number of unpopular causes, such as sex education, cremation, temperance, natural healing, and vegetarianism, and opposed capital punishment, vivisection, dueling, and vaccination.

The leaders of the peace movement ultimately failed to recognize the connection between arms limitation and international arbitration and liberalism and democracy. They could not see in their idealism that their criticism "touched the very core of the imperial political system." "The corroding influence of peace," as Treitschke called it, did not appeal to a Germany that cultivated the notion that international quarrels would most likely be resolved by violent conflict. Here Chickering emerges as a disciple of the "social imperialism" or "Kehrite" school of historiography led by, among others, Wehler and Berghahn. "The political culture of Imperial Germany was hostile to this vision of a world without war, in part because the perception of international conflict was vital to the preservation of the domestic *status quo*." In short, overseas conquests were required by Berlin in order to offset domestic political and social stress.

One wishes that the author had ended his story at this point. The chapter dealing with the French peace movement might well have been published as a separate article and the concluding section on the "antipacifists" integrated into an earlier chapter. In addition, some of the literary translations are awkward, and it is time that Chickering ended his love affair with certain pet phrases. One might also point out that there existed no monolithic German army prior to 1914; it would have been better to deal with the various particularist forces independently. Some indication of the financial backing received from the Carnegie Endowment would have enhanced this study. And might it not have been beneficial to investigate the one

occasion on which Imperial Germany submitted an international dispute (the Venezuelan blockade of 1902-03) to the Hague Court, as the pacifists so ardently desired?

To be sure, these are minor points. The work represents a major contribution to Wilhelmian studies. It is well researched in United States and German archives and complemented by judicious selection of secondary works. Moreover, the author's objectivity is remarkable. He notes that the German pacifists supported the *Weltpolitik* of Bülow and Tirpitz—and also that their counterparts in Italy championed the war in Libya, those in England expansion of the Royal Navy, and those in France recession of Alsace-Lorraine. Nor does Chickering fail to point out that German peace crusaders in 1914 went to war believing that they were fighting in the name of international justice. Yet, in the last analysis, I cannot escape the conclusion that the German peace movement was, as Carl von Ossietzky noted of Bertha von Suttner, permeated with “a gentle perfume of absurdity.”

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BARBARA VOGEL. *Deutsche Russlandpolitik: Das Scheitern der deutschen Weltpolitik unter Bülow, 1900-1906*. (Studien zur Modernen Geschichte, number 11.) [Gütersloh:] Bertelsmann Universitätsverlag, 1973. Pp. 335. DM 38.

This detailed study places Bülow's pro-Russian policy in the context of Germany's thrust for world power and demonstrates the impact of domestic policies on foreign affairs. Based on a wide range of unpublished sources from East and West German archives, the volume emphasizes two developments: the successful struggle, at home and abroad, to obtain ratification of the German-Russian trade treaty of 1904 and the meeting of Wilhelm II and Nicholas II at Björkö on July 24, 1905.

In connection with the trade treaty, Vogel shows how powerful German economic and business interests combined with Prussian dynastic traditions and ideology to influence Bülow's economic policy. The author believes that the meeting at Björkö, far from being a personal escapade of the emperor's (which is the conventional view) was carefully prepared for by Bülow and Holstein and formed an intrinsic part of the chancellor's foreign policy.

Vogel's thesis is that Bülow's aim was to attain continental supremacy for Germany through a Russo-German alliance in which Germany would be the senior partner. This aim was thwarted by the Russian refusal to ratify the Treaty of Björkö and the diplomatically unfavorable outcome of the

first Moroccan crisis. The author argues convincingly that a major factor contributing to this development was Tirpitz's reluctance to risk the fleet in a possible confrontation with the Royal Navy at the time of that crisis. As a result, Germany's goal shifted from Continental to world supremacy, including the building of a powerful navy in preparation for a challenge to Britain's hegemony. The conviction that 1904-06 was a period of lost opportunities became widespread in Germany and contributed to the decision not to allow the July 1914 crisis to pass without some decisive action by Germany.

The study is organized on a partly topical, partly chronological basis, which makes it somewhat repetitive and confusing for the reader. Aside from this it is a solid contribution to the growing literature on Germany's prewar policies, and it fills a gap in our knowledge of the Bülow period.

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ROSA LEVINÉ-MEYER. *Leviné: The Life of a Revolutionary*. With an introduction by E. J. HOBSBAWM. Farnborough, Hants.: Saxon House, distrib. by Atheneum, New York, 1973. Pp. x, 225. \$7.75.

This book, written with feeling by a widow about her first husband who was executed in 1919, needs re-evaluation in light of historical events and elucidation of facts as we know them.

Eugen Leviné was not a victim of fascism or of “evil forces.” He was not an innocent dreamer; he was a leader of the Munich rebellion to establish a Bavarian Soviet Republic and to ready Germany for the “Revolution” preached by Lenin. At the time of the Munich revolt, Leviné should have known that in Russia thousands of innocent people were being killed by the Bolshevik regime. *Reichswehr* Minister Gustav Noske, defending the Weimar Republic, had no choice but to liquidate the Munich soviets, “even at the cost of bloodshed,” to prevent the bolshevization of Germany.

Leviné and other alienated Russian intellectuals wanted to save the world, but the world did not need their means toward salvation. This is the lesson derived from this memoir: violence is always bad, particularly and also pragmatically, especially when directed toward “instant” utopia.

Leviné's life had a tragic history; he was always, so to speak, in the air, suspended in space. Socially, he belonged to the wrong movement, a fact arrived at with the benefit of hindsight.

There are some facts and spellings that warrant correction: in early 1920, a Hebrew art studio was established at the Moscow Art Theatre, which continued the work initiated in Bialystok by N.

Zemakh in 1912. This Hebrew theater, called later Habimah, was subsequently transferred to Palestine. The only pre-World War I Zionist Congress was held in Vienna in 1913. The prominent Jewish Menshevik is R. A. Abramovitch.

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ALAN F. WILT. *The Atlantic Wall: Hitler's Defenses in the West, 1941-1944*. Ames: Iowa State University Press. 1975. Pp. xi, 244. \$7.95.

General Günther Blumentritt, chief of staff in the West, after the war stated that "the reader would find it wearisome to read long descriptions of the day-to-day happenings" in France prior to D-Day. Wilt has overcome this handicap. In *The Atlantic Wall* he analyzes the three major time phases of Germany's so-called Atlantic Wall, which encompassed the Dutch, Belgian, and French coasts as well as (after 1942) the Mediterranean beaches of Vichy France: the pre-Atlantic Wall period between October 1940 and December 1941, the formulation of the defensive line between December 1941 and October 1943, and the months preceding the Allied invasion on June 6, 1944. During the latter the Germans sought feverishly to construct a formidable barrier consisting of concrete artillery installations, foreshore barriers, barbed wire, mines, and the like.

Wilt readily admits that the Western theater was secondary to the Russian front. He attributes the eventual failure of the Atlantic Wall to lack of equipment, dearth of mobile artillery and armor, interservice rivalry (especially between the army and the navy), the independent posture of Organization Todt, which poured 17.6 million cubic meters of concrete for the Wall, lack of clear-cut lines of command, and conflict in command. Concerning the last point, Wilt refutes the common notion that the quarrel concerning deployment of panzer units in France was between Field Marshals Gerd von Rundstedt and Erwin Rommel. Rather it was between the latter and the tank commander west, General Geyr von Schweppenburg, supported by General Heinz Guderian. Whatever the case, Hitler's fateful decision on April 27, 1944, to retain personal command of four of the ten armored divisions in France proved decisive during D-Day.

Wilt has relied exclusively upon captured and incomplete German records available on microfilm in Washington. This is unfortunate. A research trip to Germany would have put him in touch with several of the main characters in the book as well as the air force and navy records and personal papers in the Military Archive in Freiburg. More serious is Wilt's virtual omission of the strategic

role that Norway already had assumed by 1940 in Hitler's and Raeder's strategies; the former regarded it as the "decisive zone of this war." While Narvik and Bergen—according to Vice-Admiral Friedrich Ruge, navy liaison officer with Rommel—were protected by the entire remaining German surface fleet and 114 artillery batteries, the 625-mile coastline between the estuary of the Somme and that of the Loire was defended by a mere thirty-seven heavy guns.

This notwithstanding, Wilt's well-written and organized book represents a significant contribution toward our understanding of the German background to the Normandy and Provence invasions in June and August 1944. It is unfortunate that the typescript reproduction is riddled with typographical errors and further marred by poor typesetting and inadequate paper binding. Both Wilt's treatment of the subject and the book's price deserve better.

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HERBERT SCHINDLER. *Mosty und Dirschau 1939: Zwei Handstreich der Wehrmacht vor Beginn des Polenfeldzuges*. (Einzelschriften zur militärischen Geschichte des Zweiten Weltkrieges, number 7.) Freiburg: Verlag Rombach. 1971. Pp. 167. DM 9.

HORST ROHDE. *Das deutsche Wehrmachttransportwesen im Zweiten Weltkrieg: Entstehung—Organisation—Aufgaben*. (Schriftenreihe des Militärgeschichtlichen Forschungsamtes. Beiträge zur Militär- und Kriegsgeschichte, number 12.) Stuttgart: Deutsche Verlags-Anstalt. 1971. Pp. 439. DM 58.

KLAUS REINHARDT. *Die Wende vor Moskau: Das Scheitern der Strategie Hitlers im Winter 1941/42*. (Schriftenreihe des Militärgeschichtlichen Forschungsamtes. Beiträge zur Militär- und Kriegsgeschichte, number 13.) Stuttgart: Deutsche Verlags-Anstalt. 1972. Pp. 355.

VICTOR ISSRAELJAN. *The Anti-Hitler Coalition: Diplomatic Co-operation between the USSR, USA and Britain during the Second World War, 1941-1945*. Moscow: Progress Publishers. 1971. Pp. 422.

E. H. COOKRIDGE. *Gehlen: Spy of the Century*. New York: Random House. 1972. Pp. xxii, 402. \$10.00.

WALTER ANSEL. *Hitler and the Middle Sea*. Durham: Duke University Press. 1972. Pp. x, 314. \$12.75.

CHARLES B. BURDICK. *Unternehmen Sonnenblume: Der Entschluss zum Afrika-Feldzug*. (Die Wehrmacht im Kampf, number 48.) Neckargemünd: Kurt Vowinkel Verlag. 1972. Pp. 128. DM 17.80.

The stunning result of the German Blitzkrieg against Poland suggests a nearly flawless execution of well-planned operations. Yet the opening phase was not an entirely unqualified success. *Fall*

Weiss planners had stipulated that the twin spans across the Vistula at Tczew (Dirschau) and the twin tunnels near Mosty be captured intact prior to the actual start of hostilities. These *Handstreich* operations proved utter failures; indeed, in the latter case, the attempt alerted the Poles who destroyed the tunnels on September 1. Herbert Schindler has traced in detail these operations and has laid to rest many exaggerated interpretations of the events. He has made excellent use of the resources of the *Militärarchiv Freiburg* and other pertinent sources and secondary accounts. The Mosty endeavor, executed by an irregular force recruited by the *Abwehr*, failed because it was carried out prematurely—namely, on August 26! Equally surprising was the very conciliatory attitude of the regional Polish command in this comedy of errors. The attempt to seize the Vistula bridges was at least not compromised by Hitler's belated reversal of attack orders. In fact, he had taken a personal interest in the plans for the operation, a joint undertaking of pioneers and dive-bombers. As Schindler shows, poor timing, inadequate contingency planning, and Polish *sang-froid* combined to deny the Germans the bridges. The German sweep into Poland, however, was not significantly delayed by these failures, and by the time the campaign in the West opened, similar German *Handstreich* operations, notably against Fort Eben Emael, had proved very effective.

Both Charles Burdick and Walter Ansel turned their attention to a much more significant issue in the German conduct of the war; they examined military operations that had a decisive impact on the strategic role of the Mediterranean area. Burdick has produced a brief, well-balanced history of the genesis of *Sonnenblume* for a popular series entitled *Die Wehrmacht im Kampf*. He relied largely on excerpts of OKW, OKH, and DAK war diaries, supplemented by studies written under OCMH auspices by former German officers, and on private diaries, memoirs, and secondary works. Although it adds little to the facts, the book offers an excellent critical summary of the developments that brought Rommel and the DAK into the Western Desert. German intervention grew out of the search for strategic alternatives to a cross-channel attack on Great Britain. Proposals for action in the eastern or western basin of the Middle Sea included plans for attacks on Gibraltar, Suez, and Haifa, support of the Italians in North Africa, and diplomatic efforts to entice the Soviet government into military action in the Persian Gulf area. Räder proved the most consistent advocate of action in the Mediterranean where, with French, Italian, and Spanish support, Germany could sever the lifelines of the empire and obtain access to a vast storehouse of raw materials. The vision of solving

the British and Russian problems simultaneously hovered into focus, only to give way quickly to new and harsh political and military realities. Burdick traces the long and detailed preparations that led to the first major clash between the DAK and the British near El Agheila in February 1941. In spite of Rommel's spectacular drive to Tobruk and beyond, Halder, among others, had few illusions about the venture once the Russian campaign opened. In conclusion Burdick offers a perceptive analysis of the many problems that ultimately defeated Rommel and his forces.

Ansel, whose long and rambling account covers much similar material, largely concentrates on the conquest of Crete. He has made exhaustive use of manuscript and printed sources and has interviewed many leading figures in the German airborne assault on the island. Although he adds many factual data on the Cretan campaign, he offers no substantive reassessment of German strategy in the eastern Mediterranean. Unfortunately, the quality of the book is flawed by a writing style that has produced careless sentence structure, imprecise language, misuse of words, and excessive minutiae concerning combat operations. Yet the book has much merit in that it places the campaign into strategic perspective and examines at length the considerations that produced *Marita* and *Merkur*, neglected the complete neutralization of Malta, and affected the Russian campaign. Can scholars, however, logically deduce that missed opportunities in the Mediterranean caused the "final fatal turning," namely, the defeat in Russia, given Hitler's virtually constant preoccupation with the Soviet threat? Furthermore, I would be reluctant to support Ansel's suggestion that if only "the professionals had been left to do their job" Moscow would have been conquered by September.

Klaus Reinhardt's incisive study of the failure of Hitler's strategy in the winter of 1941–42 offers a detailed, critical analysis of *Taifun*, the code name for the attack on Moscow. The author has made meticulous use of the *Militärarchiv Freiburg* and other primary resources as well as of an impressive number of German and Soviet studies on the battle for Moscow. He offers an informative introduction on the military, economic, and political situation in the summer of 1941, and he makes clear that operational plans had assumed the destruction of the bulk of the Soviet forces west of the Dnepr. The failure to do so had far-reaching consequences both for subsequent military operations and for the armament industry, geared, as it were, for *Breiten-* and not *Tiefenrüstung*. *Taifun* was to remedy the adverse implications of the military situation. Reinhardt's account of the military operations that unfolded between September and January 1941–42 is authoritative and compelling and shows

an excellent mastery of the human and materiel factors that affected the German offensive and the Soviet defense. The growing difference between field commanders and the OKW and OKH staffs in their appreciation of the military situation is carefully recorded, as are the dramatic efforts to stabilize the front after *Taifun* had failed. Reinhardt's monograph is a major contribution to the historical literature on the Russian campaign.

In a very useful study Horst Rohde deals with matters of transportation, which assumed increasingly serious proportions during the war, especially on the eastern front. His highly systematic description and analysis of the military transport system, from an administrative and operational standpoint, is based largely on OKH records, files of the Ministry for Armament and War Production, and archives of the former *Reichsbahn*. Rohde has also made good use of a great variety of printed sources and secondary works dealing with supply and transportation affairs. He highlights the very inadequate efforts to centralize control of the transport system, the lack of a flexible and integrated air, sea, rail, and road transport structure, the excessive reliance on rail transport, and the weak political and administrative prerogatives of the transport sector. Even within the military framework no centralized authority with corresponding executive powers ever evolved during the war. Compared to the situation in World War I, the power of General Gercke, *Chef des Transportwesens*, had in fact declined. Only within the command structure of the army did he play a very significant role and exert a measure of influence on military planning. Insofar as the OKW command structure allowed, Gercke made the most of a restricted role. Little was done to extend his control over those transportation facilities not traditionally under the control of the OKH. Air and naval commands as well as civil transport authorities rarely viewed Gercke as anything other than director of army transport. Insofar as military campaigns were concerned, Gercke found it difficult to assert himself. During the campaign in Russia, he, like others, had to face Hitler's tendency to underestimate Soviet capability, a matter compounded by the failure of the *Wehrmachtsführungsstab* to note the true dimensions of the transportation problems. In his endeavor to describe and evaluate one of the less glamorous aspects of military operations, Rohde has made a very solid and needed contribution to German military history.

E. H. Cookridge, in contrast, has produced a book on military history that holds considerable fascination for a wide reading public. Moreover, Gehlen was not a man who craved or sought public acclaim, a fact that served to deepen the mys-

tery about him. As wartime chief of the German Army's *Fremde Heere Ost* (FHO) section he played the key role in the German collection of intelligence data on the Soviet Union. At the end of the war he surrendered to the Americans and placed his expertise, archives, and many staff members at their disposal. As a CIA subsidiary, the Gehlen Organization came to play a very vital intelligence role in the cold war. In 1956 Gehlen was named the first director of the Federal Republic's intelligence service, the *Bundesnachrichtendienst*, from which he retired in 1968. Cookridge, well versed in intelligence history and espionage affairs, has written a well-documented account of Gehlen's career. He had access to an impressive array of sources, not the least of which were supplied by former Gehlen associates, CIA operatives, and other members of the NATO intelligence community. Yet to speak of Gehlen as the "spy of the century" seems premature, if not naive, given the nature of the occupation. Gehlen's accomplishments are readily acknowledged, even by his adversaries; his intelligence work for the OKH and OKW, the CIA, NATO, and the Federal Republic were always highly productive. He possessed administrative flair, high professional dedication, an enormous capacity for work, and a gift for complex analytical study. To Halder, Guderian, and Heusinger, among others, he seemed to have all the qualities that Hans von Seeckt had laid down as requisites for general staff work. During the war Gehlen's estimates did not always meet with Hitler's approval, although the Führer appreciated the extraordinary abilities of the FHO chief. As the most productive of NATO's intelligence suppliers and as chief of West Germany's service, his career was not entirely untroubled. The Soviet and East German intelligence services were formidable opponents, and sloppy United States intelligence procedures did not ease Gehlen's task. Cookridge describes many of these incidents, including the Picard case that hastened the retirement of Gehlen.

Books of this type make for very interesting reading, but they usually fall far short of the true goal, namely, to account for the use to which intelligence material is put and the impact it has on the decisions made in policy formulation. Although Cookridge endeavored to present a balanced view of Gehlen's career, I take exception to the excessively flattering estimate of the accomplishments of the FHO. Critics of Gehlen in Germany have not been as kind as E. H. Cookridge.

Victor Issraeljan's book on the Allied coalition in World War II need not detain the historian in search of new Soviet interpretation or documentation. He offers the reader a traditional Soviet history of wartime diplomacy, replete with the cus-

tomary interpretations but with few references to Soviet sources. No great harm is likely to come from reading the book, a scholarly endeavor as poor in analysis as some American works of cold-war vintage.

WILLARD ALLEN FLETCHER
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GERALD STONE. *The Smallest Slavonic Nation: The Sorbs of Lusatia*. [London:] Athlone Press of the University of London; distrib. by Oxford University Press, New York. 1972. Pp. x, 201. \$12.00.

By the end of the twelfth century Slavic tribes in the area of the Saale, Elbe, and Oder rivers were either liquidated, Germanized, routed, or completely enclosed by German settlements. The Slavs of Upper and Lower Lusatia managed to survive and now inhabit a region along the Spree River from near Cottbus in Brandenburg to just south of Bautzen in Saxony. Formerly known as the Wends, a term no longer in use, these intrepid Slavs miraculously survived the horrible Nazi era, possibly because of delayed bungling by the SS, whose attention was diverted after the debacle at Stalingrad in 1943. Numerically the smallest of the Slavic peoples, numbering about 70,000 today, the Sorbs have retained their language and customs and now enjoy rights as a recognized minority in East Germany.

Stone's study, based upon interviews with Sorbian intellectuals and on-the-scene investigations, will probably remain the most complete study of these peoples. He gives equal attention to their history, literature, and language, and lesser attention to their folkways, folklore, music, and present status. Almost a curiosity or a lingering anachronism, the Sorbs (whose first serious exposure came in the late S. Harrison Thomson's chapter on them in Strakovsky's *Handbook of Slavic Studies* [1949]) must not become an endangered species. Stone's laudatory study could attract wider concern for the Sorbs and their apparent need of support for a perpetuation of their unique history and status.

SHERMAN D. SPECTOR
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HENRY KRISCH. *German Politics under Soviet Occupation*. New York: Columbia University Press. 1974. Pp. xii, 312. \$15.00.

This work relies on an extensive use of the sources available in the West (including private papers and interviews) and is in fact a detailed history of the short-lived Soviet Zone Social Democratic party, emphasizing its dialogue with the Communist party and its ultimate disintegration under Communist pressure. On the regional level Thuringian developments are featured. However, the realities

that lie outside this dialogue remain blurred, and the author does not provide details of the administrative development, the essence of the bloc policy, or the results of the factory council elections; nor can one find a convincing analysis of overall Soviet policies. Henry Krisch instead stresses attitudes and reactions, such as: "Soviet policy was carried out in the context of inherited attitudes, both German and Soviet, that influenced behaviour in 1945-1946. Above all, it must be remembered that a major and brutal military struggle between Soviet and German forces had just ended and the attitudes of that period [were] ever present" (p. 29).

Krisch does not go on to show to what extent the war experience molded Soviet postwar policy in Germany. To the contrary, he postulates a single European Soviet policy championing "national states" and favoring "all-German political institutions." Similarly, he regards a "standard Soviet policy on . . . Socialist-Communist unification" as "indisputable." But when Krisch ascribes the failure of this unification policy in Austria *inter alia* to the existence of "a single nationwide government," he leaves the reader (who recalls that Renner was instituted by the very Soviets) somewhat bewildered.

In sum, the body of this work represents a first-rate effort, which would have benefited from being put into its true perspective.

ARNOLD H. PRICE
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Konrad Adenauer, *Reden 1917-1967: Eine Auswahl*, ed. by HANS-PETER SCHWARZ. Stuttgart: Deutsche Verlags-Anstalt. 1975. Pp. 496.

HUGO STEHKÄMPER, ed. *Konrad Adenauer, Oberbürgermeister von Köln*. Festgabe der Stadt Köln zum 100. Geburtstag ihres Ehrenbürgers am 5. Januar 1976. Köln: Rheinland-Verlag. 1976. Pp. 858.

The literature on Konrad Adenauer's statesmanship is so multifaceted that it might seem as if all major aspects of his career had been thoroughly covered. The centennial of his birth brought forth, however, not only the customary solemn addresses and laudatory articles, but also some major publications that fill important gaps in our knowledge of his historical role.

After his rather involuntary resignation in 1963, Adenauer had still time and energy left to write a detailed account of his chancellorship (*Erinnerungen, 1945-1963* [Stuttgart, 1965-68]). But death prevented him from carrying out his plan to complement these memoirs by a volume of pertinent documents and speeches. Now Schwarz, who is at the University of Cologne, presents at least a selection of Adenauer's most characteristic addresses.

Many of them were not accessible to the public before. Schwarz chose them, with discriminating judgment, from about 3,000 speeches recorded in the Adenauer Archives in Rhöndorf.

The chancellor was not a brilliant orator like his contemporaries Gustav Stresemann and Theodor Heuss, but this volume impresses by the depth of his convictions and the incisiveness of his argumentation. The printed version confirms the feeling one had when listening to him: here was a statesman of dignity, forcefulness, and vision. It is fortunate that Schwarz included also some pieces from Adenauer's earlier career, beginning with his brief, but colorful inaugural address as lord mayor of Cologne (October 18, 1917).

The literature about this part of his life has been inadequate until now. Adenauer's memoirs start only in 1945, with his return to public life. The biographies by Paul Weymar (1955) and Terence Prittie (1970) give only a superficial notion of his accomplishment as chief administrator of his native town. It was, therefore, a constructive thought of the city fathers of Cologne to observe the centennial not by big festivities (such as those held in Bonn), but to honor Adenauer by a comprehensive scholarly appraisal of his career up to 1945. The editorial leadership in this task was entrusted to Hugo Stehkämper, director-in-chief of the Historical Archives of Cologne and an authority on the history of the Rhineland. He organized also, assisted by Everhard Kleinertz, a monumental exhibition in the Archives for the centennial.

Gustav Stresemann observed in 1925, having visited the magnificent Rhineland millennium exposition in Cologne, that the lord mayors (like the big industrialists) were the real kings of the Weimar era. They were elected for long terms, exerted more power than ministers, and often served also as political leaders. This remark certainly applied to Adenauer, as the memorial volume illustrates. Step by step he added new functions to his control of the city administration. He assumed a leading position in his party, the Catholic Center, especially in the Rhineland, gained key functions in the provincial self-administration, and served as president of the Prussian State Council, thus strengthening the basis of his power.

Only a few chapters in this remarkable volume can be mentioned here. Everhard Kleinertz gives a good account of Adenauer's training for leadership in the eleven years he was a member of the Cologne city administration, before being elected lord mayor. Marie-Louise Becker examines his relationship with the British occupation authorities after World War I. Friedrich-Wilhelm Henning analyzes his financial policies with fairness, but not uncritically. One of Adenauer's finest accomplishments, the revival of the University of Cologne

right after the November revolution, is described lucidly by Kurt Düwell. Ekkhard Häussermann presents fascinating new material about Adenauer and the press, especially his unusual friendship with his Social Democratic political opponent Wilhelm Sollmann, to which I had recently called attention in my essay on Sollmann in *Rheinische Lebensbilder* (vol. 6, 1975). The most important chapters for historians are by Stehkämper himself and by Rudolf Morsey, the leading expert on the Center party. Stehkämper deals with Adenauer's two unsuccessful candidacies for the chancellorship. In 1921 his own party elders preferred Joseph Wirth; in 1926 Stresemann was not keen to work with such a strong-willed man as head of the cabinet. Who dares to guess how German history would have been shaped if Adenauer had been able to form a government in 1926? Morsey's chapter on Adenauer and National Socialism is enlightening; using much previously unknown material, he demonstrates the incompatibility of his political philosophy with the outlook of Hitler and his henchmen. He also tells how Adenauer suffered for his convictions under Nazi rule. Out of the terrible experiences of these twelve years grew his determination that postwar Germany should have an ethical basis for its policies.

The carefully edited, richly documented, and well-illustrated volume is the model of a modern city history. It is commendable that the contributors did not produce a piece of hagiography but a judiciously balanced scholarly work that shows both the greatness and some limitations of Adenauer's administration of Cologne.

FELIX E. HIRSCH
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ROBERT A. KANN. *A History of the Habsburg Empire, 1526-1918*. Berkeley and Los Angeles: University of California Press, 1974. Pp. xiv, 646. \$25.00.

Robert A. Kann is the doyen of the American historians interested in the lands of the Habsburgs. The volume under review is the most recent in a long line of distinguished books and, at least in one sense, it is the most comprehensive of all of them. It goes beyond the traditional Vienna-centered narrative of the fortunes of the Habsburg family and gives us a history of the numerous people who lived under the scepter of this historic ruling house.

The initial date, 1526, is well chosen. The election of Ferdinand I to the thrones of Bohemia and Hungary in that year, followed in January 1527 by a similar event in Croatia, drastically changed the fortunes of these three peoples, but also determined that of the dynasty for roughly 400 years. It made the Habsburgs responsible for conducting

the protracted struggle against the Ottomans, turning their attention gradually away from their traditional field of action in Western Europe to their newly acquired possessions. By 1918, when the Habsburg Empire, then known as Austria-Hungary, collapsed, it consisted chiefly of these lands and the "hereditary provinces" (approximately present-day Austria) of the ruling house.

The various nations that first came entirely or partially under Habsburg rule in 1526–27 had long histories, traditions, laws, and languages of their own. Now they had to adjust to a "foreign dynasty," to the inevitable changes required by this very fact, to the absence of the ruler from the traditional capitals, and to the necessity to fit into a dynastically and politically dictated over-all scheme of which they were only a part. In many ways, the history of this new Habsburg domain is that of the successes and failures of peoples and rulers in understanding each other and working out a mutually satisfactory *modus vivendi*.

The work is organized basically along chronological-political lines. The first chapter gives the short, needed background for both the "hereditary provinces" and those lands that came under Habsburg rule in 1526–27. The fusion of these units created problems because the various lands brought different political practices and institutions into the "union." Overcoming these differences was made more difficult by the simultaneous difficulties raised by an external problem, the Ottoman danger, and by a domestic issue, the Reformation. The second chapter, ending with 1648, treats these three closely interrelated issues.

The next hundred years, discussed in the third chapter, saw the end of the Ottoman danger and the practical solution of the religious problem, but also witnessed the emergence of new problems as a result of the incomplete integration of the realm. They end with the ascension of Maria Theresa to the throne and the beginning of the War of the Austrian Succession.

The rule of Maria Theresa was a turning point in the history of the Habsburg lands. Having reached this point, Kann stops in his narrative and devotes a long, very interesting chapter to reviewing the socioeconomic trends, the estates, absolutism, Church-State relations, culture, and the administrative-judicial-defense systems during the years whose political history was the subject of the previous chapters.

The fifth chapter, taking the reader to 1815, is a combination of the political-narrative approach of the first three chapters and the analytical one of the fourth. Dealing with the reform attempts of the queen and the more radical approach of her son, Joseph II, this approach is more than justified. The various trends are so closely interwoven and

influence each other to such a large extent that the only acceptable approach is the one selected by the author, analytical-narrative.

The title of the next section, "Standstill, Decline, and Stabilization," bringing the story to 1879, is once again basically narrative. While correct in every detail and clearly presented, however, the chapter suffers from a relative overemphasis on the one hand and not enough attention given to other issues on the other. The next chapter dealing with the Enlightenment and liberalism was more successful. Here Kann's familiarity with the people of the monarchy and his understanding of them and their problems showed to great advantage.

The next two chapters take the chronological developments from 1879 to 1918. Good, solid history is presented in this part of the volume, but the treatment of these well-known years and events hardly lends itself to new interpretations. The last chapter brings cultural history up to 1918. Once again Kann proves that he understands his topic well. Together with the previous chapter devoted to this topic, this last one gives us the best short survey of the subject in English.

The volume ends with a bibliographic essay, useful appendices, and a set of helpful maps. The work before us will certainly enjoy the popularity of the author's previous volumes and will serve as a basic history and reference book for many years to come.

PETER F. SUGAR
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CLAUDE TAPPOLET. *La vie musicale à Genève au dix-neuvième siècle (1814–1918)*. (Mémoires et documents, 45.) Geneva: Alex. Jullien publiés par la Société d'histoire et d'archéologie de Genève, 1972. Pp. 215.

Claude Tappolet, author of the important account of modern Genevan musical life which appeared in the collaborative *Histoire de Genève* (1956), has now intensified his treatment of the subject in the present volume, one in the excellent series published by the Société d'histoire et d'archéologie de Genève. The basic thrusts of his narrative have remained the same: the rise of musical institutions; the contributions of gifted figures to the accelerating tempo of Genevan musical life; and the sustaining of that culture in part by an international community of French bankers, well-heeled emigrés, English gentlewomen and consuls, and the occasional political radical. The author's story is a progressive one—from the beginnings of musical life in voluntary, often secular organizations, through the founding and consolidation of the Conservatoire, the seconding of that in-

stitution's democratic impulse by cheaper public concerts, the appearance of a native school of composition, and the resiliency manifested by some of these institutions during the Great War.

In Tappolet's account the pathway to success of many contemporary Genevan musical organizations is littered with the exhumed remains of an extraordinary assortment of performing societies, most of which were the short-lived projects of some of the great *animateurs* of musical life. To the efforts of these men—and he shows how many were German or French—Tappolet pays generous tribute, and he sets forth the strengths and weaknesses of their aborted dreams.

In keeping with the format of the series to which this volume belongs, the author includes a collection of documents. Particularly interesting to the student of other Continental and English musical institutions is Tappolet's reproduction of the codes of regulations of the major successful Genevan societies. Here strike the notes, as they could not within the confines of his 1956 sketch, redolent of the problems of voluntary organization and artistic professionalism interacting within and with a society at once proudly knowledgeable of its republican heritage and proudly Swiss; at once increasingly secular, liberal, and occasionally radical, and yet deferential to the place owed the teachings and the officialdom of the Reformed religion.

It is disappointing that Tappolet largely fails to elucidate most social, political, and economic dimensions of musical life. I cannot but expect that such fruit might be yielded by the fine archives that Claude Tappolet has exploited well for his narrower purposes.

DAVID WARREN HADLEY
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J. G. A. POCOCK. *The Machiavellian Moment: Florentine Political Thought and the Atlantic Republican Tradition*. Princeton: Princeton University Press, 1975. Pp. x, 602. Cloth \$22.50, paper \$11.50.

In this major work, John Pocock's analytic sophistication and formidable scholarship are employed on the tradition of "civic humanism"—a large subject. The story Pocock tells is how Machiavelli and other Florentine political theorists grappled with the problems of understanding and establishing institutions that would enable men to fulfill their natures as citizens (thus realizing eternal values) at a particular moment in time and therefore in specific conditions. The available concepts were severely limited, for time could be seen either as a sequence of experiences in a particular com-

munity, "custom," or alternatively a temporal instant could possess universal significance as an intrusion of the eternal, "grace."

Their answer was a mixed or balanced government allotting to the prudent, ambitious "few" and the numerous "many," functions suited to their natures (as had Aristotle); there might also be a leading, unifying "one" (as in Polybius). But the constitutional republic is a finite, "accidental" occurrence, achieved only in favorable circumstances, subject to external destruction by "providence" or "fortune" and to internal corruption if its citizens' virtues decay. For Savonarola and some of God's seventeenth-century Englishmen the virtuous republic could only result from grace given to a chosen people. Machiavelli explored the possibility of creating and renewing civic virtue through a variety of institutions, such as a militia; yet the *virtù* of leader or people was always threatened by time—the vicissitudes of *fortuna* and the tendency of success to cause corruption. Guicciardini advocated a larger role for the prudent and experienced few in view of the uncertainty of external affairs. For some, Venice provided ways to stabilize the republic and stop the wheel with a balanced machinery of government.

Thus we have arrived at part 3. Part 1 (pp. 3–80) sketched the notions of custom, grace, and *vivere civile*; part 2 (pp. 83–330) examined Florentine thought from 1494 to 1530; part 3 tells us how Englishmen (and North Americans) came to think of themselves as Machiavellian citizens notwithstanding their customary, ancient constitution and their puritan commitment. Pocock eschews the "rise of the bourgeoisie" and the "revolution of the saints," discovering a development through the 1640s and '50s to that moment in which James Harrington could reconceptualize English history and political thought. Later, Harrington's theory of the militant popular commonwealth was turned upside down to justify a country opposition, the House of Lords, and the ancient, Gothic constitution, and to attack standing armies, the "monied interest," stockjobbers, Walpole, and corruption. American patriots imbued with these ideas could not avoid seeing their liberty threatened by British corruption. Thus "the Revolution was paradigmatically determined and an essay in Kuhnian 'normal science'" (p. 508). Moreover, Madison's *Federalist 10* and other American views can be seen as rearrangements of this pattern.

A work of this scope is bound to raise problems. Here I can touch only a few. First, what is the status of the conceptual apparatus deployed? Are these ways of political understanding specific to this tradition? Or are they (as Pocock's Chinese allusions suggest) general categories? The ideas discussed are occasionally called paradigms, or a

paradigm (Kuhnian?), sometimes called vocabularies or a political language, frequently modes of rendering things intelligible. (This list may not be exhaustive.) Their epistemological and ontological status needs further examination.

Moreover the book is written in several manners: part 1 explains and illustrates typical ideas, for example, "custom" by Sir John Fortescue. Texts are mentioned but rarely quoted. Part 2, however, engages in extensive detailed, even line-by-line, exegesis accompanied by quotation by the paragraph in the original language of the text. Thus the argument is microscopically focused on particular strands in the thought of Machiavelli and others. In part 3 the focal distance is again changed. We are urged to reorder our perceptions and are seldom brought to consider a text closely. In the final chapter our perceptions are so swiftly shifted that we may be dazzled by proliferating moments (Machiavellian, Rousseauist), multiplying men (*politicus, rhetor, credens, mercator, creditor, and faber*), and the third dark apparition of the problem of modernity. At one point "commerce," previously a secular analogue of *fortuna* since it leads to luxury and therefore corruption, is identified with *virtù* because both are innovatory. (A is C; B is C; therefore A is B.)

Usually Pocock invites us to consider the possibilities inherent in a set of ideas. But sometimes one is puzzled by his account of a thinker. Is he telling us what Aristotle (pp. 66-80) meant, or what he could mean, or how some interpreters understood him? Is Pocock's Aristotle merely exotic, or minced up and reconstituted by his intellectual machinery? Occasionally one fears that the huge apparatus is running wild or being converted into a steam roller cracking nuts.

Most importantly Pocock explicitly raises the serious problem of the relation between thought and action. How much of men's politics is predetermined by the politics of their language? Some situations (soldiers' pay in 1647, the conflicts of the 1780s in the not yet United States) are submerged beneath the dialectic of ideas. Yet Pocock is no idealist.

The Machiavellian Moment thus gives a magisterial account of an important tradition and challenges us to consider a number of major problems. Even those who disagree with his views or quarrel with his interpretations cannot avoid joining me in applauding John Pocock's *virtù*.

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PHYLLIS WALTER GOODHART GORDAN, translated from the Latin and annotated by. *Two Renaissance Book Hunters: The Letters of Poggius Bracciolini to*

Nicolaus de Niccolis. New York: Columbia University Press. 1974. Pp. x, 393. \$17.50.

This book is a labor of love. Only one who is steeped in the traditions and language of the Italian Renaissance and familiar with and tolerant of the arch and artful posturings of the Italian humanists, who respects their accomplishments and recognizes the innumerable *combinazioni*, could have produced it. It is not surprising to discover that its production covers a period of some forty years. This is not a work that could have been done in one, two, or five years, nor is it one that should be read from cover to cover in a few sittings.

Two Renaissance Book Hunters is a reference work. It is fully entitled to join its illustrious companions in Columbia's *Records of Civilization*. Phyllis Gordan may rest assured that generations of students will find her work useful and usable. It is, perhaps, appropriate that the book started out as an undergraduate term paper and became a lifelong obsession.

The one hundred letters of the prolific Poggius Bracciolini represent only a part of the work of the Florentine humanist. The selection translated here is a good one, exemplary of the enthusiasm and inspiration of the "second generation" of collectors of ancient manuscripts, as well as of their occasional pettiness and not infrequent naiveté. The translation is a major accomplishment because it has captured the interlinear thought of Bracciolini, wisely translating his idioms not literally, but with a splendid understanding of the niceties and *double entendre* of Tuscan literati.

Ninety-three of the letters are from Poggius to Niccolò Niccoli; one is a dedicatory letter to Francesco Ferrari; eight are to and from other collectors. These last, appearing separately in an appendix, provide fine firsthand information about discoveries of new manuscripts and new meanings assigned to old ones.

When the work was undertaken, complete or near-complete collections of Bracciolini's letters were rarer than the Gutenberg Bible. Now, thanks to modern technology, additional copies are available. Even so, the translation is one that will have many uses.

There are only two criticisms offered, and they are minor. The insistence on the use of the classical Latin versions of names can get out of hand after a while. One can become accustomed to Poggius rather than Poggio; to Nicolaus rather than Niccolò. But Cosmus and Laurentius de Medicis? And if this be so, why Jerome (p. 174) instead of Hieronymus? As they say in Tuscany, "Macchè!"

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DEBORAH HOWARD. *Jacopo Sansovino: Architecture and Patronage in Renaissance Venice*. New Haven: Yale University Press. 1975. Pp. xi, 194. \$18.50.

This richly illustrated and accurately researched study is a comprehensive survey of the artistic career of Jacopo Tatti, surnamed Il Sansovino (1486–1570), in Venice after 1527, the year in which he fled Rome. The subsequent forty-three years, from 1527 to 1570, constituted, for Sansovino, a most creative and remarkably active experience. The artist's architectural activities were favored by the fortuitous combination of various circumstances: peace, security, and economic stability. They were further enhanced by a concern on the part of the city's fathers and the Venetian aristocracy with pomp, ostentation, and innovation. The latter justified the undertaking of a city beautification plan.

In six concise but informative chapters the author has presented a perceptive and accurate account of the numerous artistic commissions that Sansovino executed for various Venetian patrons: namely the procurators of St. Mark's, the state, the Church, the *scuole* or charitable institutions, and the Venetian patriciate. Indefatigable, imaginative, and resourceful in proposing architectural plans that could best suit his patrons' economizing considerations or artistic tastes, Sansovino was responsible for the construction in Venice of many impressive structures—for example, the library, the mint, the *Loggetta*, the *Fabbriche Nuove di Rialto*, the Palazzo Corner, and the Church of S. Francesco della Vigna. By this array, he left an indelible imprint on Venice's secular and ecclesiastical architecture, earning for himself a reputation that can be matched only by Pietro Lombardo, Palladio, and Longhena.

The idea that a city should represent a work of art was first conceived in Renaissance Italy. Its first successful experiment—the resultant of papal munificence and Bernardo Rossellino's architectural genius—was carried out in the Tuscan hill town of Pienza. The city, however, that exemplified the idea of town planning at its best was sixteenth-century Venice. Among the talented men who strove for the realization of this concept, Sansovino, as *Proto dei Procuratori di Supra* (architect-in-chief to the Procurators of St. Mark's), acted as an inspiring catalyst.

Deborah Howard's basic argument is simple, logical, and convincing. In refuting the contentions of many scholars who have denied that Sansovino developed a personal style, she maintains that the reverse is true, although economic restrictions and other controlling factors did "almost completely obscure the architect's own inclinations." If it is true that Sansovino's style evades any classifica-

tion, then, on the other hand, an analysis of his architectural structures reveals the progression of a personal style.

Sansovino's style does indeed exhibit peculiar features, such as compliance with Vitruvian orthodoxy, structural elegance, and reliance on the Roman High Renaissance style. It reflects the influence of other master architects, such as Bramante, Raphael, and Sangallo. It also shows a penchant for some favorite motifs, such as the rusticated basement surmounted by coupled columns, the elegant corner solution, the blocked balustrade, the oval Attic windows, and the long consoles flanking the mezzanine windows.

The value of the study is enhanced by informative notes, a detailed index, and a number of rich plates that add a visual dimension to the narrative. A model of felicitous writing, this monograph is characterized by an absence of factual and typographical errors. In conclusion it suffices to observe that every thoughtful student or specialist will find this study an invaluable reference work. Furthermore, it is a stimulating and scholarly monograph that fills an extant lacuna concerning Sansovino in English-speaking countries.

THOMAS A. FABIANO
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FRANCO DE FELICE. *L'agricoltura in terra di Bari dal 1880 al 1914*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1971. Pp. 502.

ALDO DE MADDALENA. *Prezzi e merci a Milano dal 1701 al 1860*. Volume 1; volume 2, *Grafici*. (Studi e ricerche di storia economica italiana nell'età del Risorgimento.) Milan: Banca Commerciale Italiana. 1974. Pp. 455; 99 graphs.

These two well-documented studies in Italian economic history are important additions to the series of studies promoted by the Banca Commerciale Italiana of Milan and its late president, Raffaele Mattioli.

Franco De Felice's study fills an important gap in the literature and provides valuable historical material on the problem of underdevelopment in southern Italy. Agriculture in southern Italy suffered a long feudal inheritance. It was a backward, overpopulated sector with low productivity; it lacked capital, technology, and markets in the nineteenth century; and it was able to survive only with the intervention of the state. Eighty percent of those employed were agricultural day workers. The *meridionalisti*, as De Felice shows, did not have a comprehensive grasp of the problems of Italian agriculture in the 1880s—the real problem was not to protect agriculture in the south, but to trans-

form and modernize it through the introduction of new crops, fertilizers, agricultural schools and technical instruction, machinery, livestock raising, and so on. De Felice focuses on the relationship between economic backwardness and productive relations in agriculture. One of the main roots of southern underdevelopment, he points out, was the conflict between crop patterns and technology and the various forms of land tenure systems and national economic policies. This is in sharp contrast to the development of a modern capitalistic sector in the north.

Aldo De Maddalena has contributed a monumental study on prices and wages in Lombardy between 1701 and 1860 that will be fundamental for all future work on the history of the economy and society of northern Italy in the preunification period. His study presents annual price series for twenty-seven commodities, predominantly agricultural products and foodstuffs, but also prices of manufactured goods, industrial raw materials, colonial products, and services (rents) for a period of 159 years. The wage series covers, unfortunately, only two grades of labor (bricklayers and apprentice bricklayers). Price data have been compiled from primary sources, largely from the registers of the Archivio dell'Ospedale Maggiore and from the Archivio del Pio Albergo Trivulzio in Milan. The results are summarized in the form of annual arithmetic averages and index numbers for individual commodities. Moving averages have been constructed for commodity groups for analysis of long-term trends. The study contains fifty-eight tables and ninety-nine graphs.

Agricultural prices in Lombardy show a rising trend, allowing for notable fluctuations, between the 1730s and 1830s. Industrial prices are stable through the greater part of the eighteenth century—a period of economic depression in manufacturing, little population growth, and depreciating currency. Industrial prices (primarily textiles) begin to decline in the 1830s with tariff reform and cheap imports from abroad, but they rise in the 1850s with the beginning of large-scale industrialization in Lombardy. In general, prices rise faster than wages between 1701 and 1860, indicating a steady erosion of purchasing power among consumers and the wage-earning class. Wages maintain a stable level through the eighteenth century, rising modestly only during the early decades of the nineteenth century. The extreme rigidity of the wage index in the eighteenth century, however, suggests the possibility that nonmonetary factors (payments in kind, fixed contractual rates, and so forth) may have influenced the behavior of money wages. One can barely begin to do justice, however, to the wealth of detail and the complexity of the study in a short space. De Maddalena con-

cludes from this analysis of prices and wages that the secular process of capital accumulation in Lombardy was accomplished largely at the expense of the laboring class.

One hopes that with De Maddalena's work at hand, historians will turn to the study of long-term movements in production, consumption, demographic growth, and income distribution, which accompanied these fluctuations in prices, so that we may ultimately have a comprehensive profile of the social and economic structure of one of the most economically developed regions of Europe in the preindustrial period.

IRA A. GLAZIER
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PAUL CORNER. *Fascism in Ferrara, 1915-1925*. (Oxford Historical Monographs.) New York: Oxford University Press, 1975. Pp. xii, 300. \$25.75.

Paul Corner's monograph is a successful attempt to examine the circumstances that gave rise to the Fascist movement and permitted its survival in a key province of the lower Po Valley, Ferrara. The author rightly insists that it is important to study provincial Fascist organizations, for without his provincial bases, Mussolini probably could neither have come to power in 1922 nor remained in power during the protracted crisis that followed the assassination of Giacomo Matteotti in June 1924. Moreover, the provincial Fascist movements exerted much influence on the formation of the Fascist ideology. Corner warns his readers, however, that the experience of Ferrara is not to be taken as representative of that of most other provinces. Ferrara was clearly exceptional, simply because it was one of the first areas to become Fascist and did, to some extent, lead the movement rather than follow it.

For several crucial months in 1921 the province of Ferrara was the most vigorous element in the developing national Fascist movement. It was Ferrara that formed the spearhead of the rapid expansion of reactionary agrarian fascism that effectively rescued the town-based fascism of Mussolini from political extinction. Although the province never assumed quite the same degree of importance in later years, it nevertheless continued to be a Fascist stronghold. Furthermore, it was in Ferrara that one of the most prominent Fascist leaders, Italo Balbo, made his name and established his political base. Thus, many of the themes central to the problem of the rise and subsequent stabilization of fascism are found in Corner's study of this province. Certain of them relate to the methods and organization of fascism—for example, the systematic employment of violence by the *squadristi*;

the establishment of Fascist syndicates as a means of controlling the agricultural laborers; and the clarification of the structure and decision-making machinery of the provincial Fascist federation. Others concern the splits that emerged within the local Fascist movement, as well as the growing tendency for fascism to become completely intolerant of criticism, either from without or within. Such squabbles often reflected the diversity of social origin that existed between Fascists who came from the towns and those who came from the rural areas.

Corner's study concentrates chiefly, however, on the origins of the Fascist movement. Ferrara was remarkable for the speed with which fascism asserted its control. During the postwar years of 1919-20 the province had become one of the bastions of revolutionary socialism; yet it took only weeks for the entire socialist organization to be brought to submission. Why? Violence was, of course, part of the story; but the principal factor, according to Corner, was the economy of the province. Ferrara was notable at this time for its modern capitalist systems of agriculture; yet, side by side with these advanced forms of farming were other systems that had changed very little over the centuries. The tensions arising from this contact of old and new were a real factor in provincial life—especially for those small farmers, leaseholders, and sharecroppers who felt most threatened by the growing agricultural proletariat. Thus, the real struggle in Ferrara was not so much between Fascist *squadristi* and members of the socialist and Catholic leagues as it was between the hard-pressed small proprietors and what for them were the undesirable consequences of modernization in agriculture. Corner argues that it was these landowners who were the key to the provincial struggle. The *squadristi* were a necessary element in the maneuver against socialism, to be sure, but once that maneuver was over, they ceased to have much relevance to the local situation. Balbo, for example, rapidly became disinterested in politics and turned his attention to aeronautics.

Originally a doctoral dissertation prepared under the supervision of Christopher Seton-Watson at Oxford, Corner's monograph is based largely on material found in the Archivio Centrale dello Stato, particularly the archive of the Ministry of the Interior, as well as on local and national press sources, secondary works, and some interviews. The author has also profited from suggestions by Adrian Lyttelton, Stuart Woolf, Renzo De Felice, and Giuliano Procacci. Some sections of Corner's book are unfortunately cluttered by an alphabet soup of abbreviations of obscure organizations. A glossary would have been very helpful. One hopes that Corner's otherwise excellent book will inspire

English-language studies of other provinces that figured significantly in the rise of Italian fascism.

CHARLES F. DELZELL
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APOSTOLOS E. VACALOPOULOS. *The Greek Nation, 1453-1669: The Cultural and Economic Background of Modern Greek Society*. Translated by IAN and PHANIA MOLES. New Brunswick, New Jersey: Rutgers University Press. 1976. Pp. xiv, 457. \$25.00.

The scope of this formidably learned book is accurately indicated by its full title. Apostolos Vacalopoulos is interested in the antecedents of the Greek nation as defined in the nineteenth century, and he distributes attention to legal, political, economic, religious, sociological, and geographical matters in successive chapters. The book assumes full familiarity with the course of events—political, military, and cultural-religious as well. Thus, for example, despite a chapter on religion and another on education, Vacalopoulos has absolutely nothing to say about the three-cornered propaganda struggle among Roman Catholics, Protestants, and Orthodox churchmen that dominated the diplomatic stage in Constantinople during the first decades of the seventeenth century. What attracted the author's attention instead was an enormous range of items recording steps along the way toward making Greeks more like the Greeks of, say, 1821 or perhaps 1920, before Hellenism collapsed in Asia Minor.

The main use such a book can have for American readers is as a source for bits of detailed information. Indeed, the massive annotation occupying more than one-third of the volume, drawing on nearly a dozen languages, will be for many the most valuable part of the work.

Where Vacalopoulos' scholarship fails is at a conceptual level. Retrojection of nationalist classification of human activity into the Ottoman past invites enormous confusion, because such ideas were alien to the society of the time. Vacalopoulos never explains the ambiguity between "Greek" and "Orthodox Christian" that lies buried in the language he uses. He furthermore assumes rather than says that Orthodoxy was unchanging and so simply does not mention the complex and delicate doctrinal issues that constituted the kernel of "Greek" intellectual history in the sixteenth and seventeenth centuries.

Such selectivity faithfully represents views that have an almost sacred status in modern Greece; yet to an outsider they seem to have blinders that obscure the very real effort Vacalopoulos has made to maintain a cool and objective tone in men-

tioning Turks, Bulgarians, and other traditional national rivals.

WILLIAM H. MCNEILL
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EVANGELOS KOFOS. *Greece and the Eastern Crisis, 1875-1878*. Foreword by W. N. MEDLICOTT. (Institute for Balkan Studies. Serial, number 148.) Thessaloniki: Institute for Balkan Studies. 1975. Pp. 283.

Kofos has given us a sample of microhistory at its best. He deals with the Eastern Crisis of 1875-78 when the so-called powder keg of Europe, the Balkans, went through a sort of controlled explosion—controlled thanks to the determination of the major powers to prevent the conflict from driving them into a direct armed confrontation with each other. The crisis was triggered by the Russian drive to establish a strong Bulgarian state extending from the Danube to the shores of the Aegean. When the Russian armies defeated the Turks in the spring of 1878, the sultan was forced to sign the Treaty of San Stefano, which created a huge Bulgarian state. Greece was faced with the danger of being relegated to a small area in the southern tip of the Balkan peninsula, a fate that she escaped but not because of the policies of her leaders. During those critical months Greece was practically leaderless. A coalition government that included all former prime ministers, under the leadership of the revolutionary hero Kanaris, then in his nineties, was formed in May 1877. Kanaris died in September, and for the next four months none of the participating leaders would accept a replacement for the prime minister's post. Greece escaped the worst mostly because of the intervention of the British, and to a lesser extent of the French and the Austrians, who were none too happy to have a strong Russian satellite so close to the Straits.

The highlights of those momentous years have been recounted numerous times by contemporaries and by more modern historians. Most of those writers studied the crisis in the context of international diplomacy, but little attention was paid to the role played by the nationalities in the area. Kofos' valuable book does not ignore, of course, the twists and turns of international diplomacy, but it also illumines the policies and actions of the Balkan nationalities, especially the Greeks. He analyzes both official Greek policy—or the lack of it—as well as the impact of the revolutionary ferment in Crete, Thessaly, Epirus, and Macedonia.

The book offers no startling revelations that would require an extensive revision of our knowledge or evaluation of the events of 1875-78. But Kofos, in clear prose, restructures in detailed relief

the behind-the-scenes exchanges, maneuvers, intrigues, misunderstandings, or cherished illusions that so often are the unseen building blocks of history. He does so by meticulously probing into the records, the diplomatic dispatches, the governmental papers, and the confidential assessments of those directly involved.

His findings should also be of interest to those concerned with today's political scene in Greece. Although the events discussed in this book occurred one hundred years ago, they offer valuable insights into the weaknesses—the lack of a coherent strategy, the internal contradictions and feuds—that even today afflict the making of Greek foreign policy.

D. GEORGE KOUSOULAS
Howard University

HUGH SETON-WATSON. *The "Sick Heart" of Modern Europe: The Problem of the Danubian Lands*. Seattle: University of Washington Press. 1975. Pp. xi, 76. \$4.95.

Hugh Seton-Watson's slim volume is made up of three lectures delivered at the University of Washington in Seattle in 1973. The author presents the problems of East Central Europe, to him the "sick heart" of Europe, in three phases. The first is that of the three declining old empires, Austria-Hungary, Germany, and Russia. The second phase covers the interwar period, "the interlude of small states," and the third that of "the new empire," the Soviet Union. All three lectures exhibit the author's well-known qualities at their best: the ability to summarize, simplify, and arrive at comprehensive deductions.

The volume reveals, however, the reverse side of these assets as well, such as the tendency to ride roughshod over facts that may stand in the way of well-formulated and seemingly convincing conclusions and a preference for oversimplifications. It appears problematic to compare on a par three empires of which two, Germany and Russia, continued to exist in changed form after 1918 while the third, the Habsburg monarchy, dissolved. The initial statement repeated later in the text that "all three empires, it may be argued, were replaced by barbarism," from which Seton-Watson only exempts Weimar culture and Masaryk's Czechoslovakia, while in several respects correct, is nevertheless excessive when expressed so categorically. This thesis definitely does not hold true, to mention a few instances, for the early stages of the Austrian Republic, for the first years of restored Poland as compared to the last ones in the best part of partitioned Poland, and for the Romanian administration of Transylvania as compared to the Hungarian. It is an oversimplification also, though a less serious one,

to state that the basis of "legitimacy" in the western, Cisleithanian part of the Habsburg Empire was simply *Kaisertraue*. In contradiction to this, the author subsequently recognizes a number of centripetal forces that supported the monarchy; here his analysis follows in substance that of Oscar Jászi. Furthermore, is it really necessary to establish a highly subjective scale of higher and lower levels of culture among the national groups in the Habsburg Empire that may antagonize many but will benefit hardly anybody? As a last example of the author's problematic oversimplifications, according to his account of the German-Polish crisis of the summer of 1939, Hitler believed he had made the Poles "a very generous offer. They need only give him some small bits of territories and then they could be his partners in conquering the Ukraine." Here the basic distinction between preservation of territorial integrity (plus the award of crumbs as bonus) and the loss of genuine independence and reduction to satellite status is missed. Hitler was obviously quite aware of it.

This very readable volume definitely contains many apt observations, some of which modify preceding generalizations. The basic philosophy is tenable as well. Yet, readers would benefit more if, in his future works, Seton-Watson would divert his great gifts from the sweeping and didactic to the more specific and factual.

ROBERT A. KANN
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REUBEN AINSZTEIN. *Jewish Resistance in Nazi-Occupied Eastern Europe: With a Historical Survey of the Jew as Fighter and Soldier in the Diaspora*. London: Paul Elek; distrib. by Barnes and Noble Books, New York. 1975. Pp. xxviii, 970. \$37.50.

Was Jewish resistance to the Nazis fact or fiction? A scholarly work describing the facts and analyzing the problems would satisfy a real need. Unfortunately, Reuben Ainsztein never quite makes clear what it is that he is discussing: resistance of Jews who may have felt as Russians, Frenchmen, and so on, or the resistance of individuals and groups arising out of some kind of Jewish identification or motivation, namely a specifically Jewish resistance. Nor does he define what he means by resistance; he simply lists underground activities and escapes, later called "elemental resistance," alongside armed actions. Yet he nowhere describes cultural, educational, economic, or religious underground actions taken in defiance of Nazi laws. His account of underground activities directly leading to armed resistance (in Warsaw, for instance) is even more inadequate. Worse, he never analyzes the problems, nor does he draw any conclusions from his facts. The result is a vast compilation of

facts that tend to become tedious because there is no conceptual thread running through them, beyond the desire to prove that Jews could fight. I doubt whether 970 pages were required to prove that.

The first 212 pages of the book are designed to disprove the charge that Jews in the Diaspora had no martial traditions; Ainsztein produces an array of facts covering nineteen centuries, completely devoid of any analysis and removed from context. Some facts are well documented, some are not; no attempt at differentiation is made. Ainsztein's antireligious bias prevents him from probing Jewish traditions for undercurrents that might prove his point. The results of his quest can only be described as chaotic.

There then follows a summary of the "final solution," some comments on "elemental resistance" through flight, and, unaccountably, the revolts in small-town ghettos (pp. 215-75). A major part of the book (pp. 279-460) describes Jewish partisans in Eastern European forests. Ainsztein deals with ghetto rebellions in parts 6 and 7 of the book (pp. 463-681), and then death-camp rebellions (pp. 685-816). A summary chapter merely adds Jewish armed action in two more places and does not sum up anything.

A vast amount of labor has produced an occasionally imposing array of facts, which are unfortunately overshadowed by the way Ainsztein uses his sources. Soviet claims, memoirs, and so on are accepted as true; anti-Semitism among Soviet partisans is hotly denied, despite overwhelming evidence to the contrary; and religious Jewry is denigrated. Ainsztein uses almost no archival sources, excepting some materials of the Jewish Historical Institute at Warsaw. He ignores the 30,000 testimonies at the Yad Vashem Archives in Jerusalem (except for one collective testimony about Sobibór), and he ignores other archives in London, New York, and Israel. Having limited himself to published sources, the author does not use Hebrew publications at all, except two old compilations and some incidental materials. Journals such as *Yalkut Moreshet* or *Yad Vashem Studies* and literally hundreds of memoirs and books published in Hebrew are indispensable for a study of this subject, but Ainsztein has passed by them.

Despite the tremendous volume of facts, some major events simply do not appear. Armed attempts at rebellion in Częstochowa, Tarnów, and Bendin and at Kielce, Opatów, Pilica, and Tomaszów Lubelski, the attempted revolt of the Jewish police at Riga, the underground in Siauliai, organized escapes to the forests from Radzyń, Rzeszów, Chmielnik—the list could be extended—all are ignored, as are the rebellions in the camps at Kruszyna and Krychów. The ac-

count of the Treblinka rebellion (pp. 714–42) is not very accurate or convincing, but in a footnote on page 916 the author quotes at length an account published in 1933 that changes the picture and that came to the author's notice too late to be included in the text. He bases his claim on page 221 that the Nazis discussed extermination of Jews and Slavs (*sic*) before the end of 1940 on an obscure publication of 1962 that avoids quoting its sources, but a check of Charles Wighton's book on Heydrich showed no record of any such discussion. There are numerous instances of this sort of thing.

The story of Kovno is an example of the author's treatment of his subject (pp. 699–701). He claims the Judenrat, whose chairman, Yochanan Elkes, becomes Dr. Alex in the book, was compliant. Ainsztein then contradicts this by saying that the Judenrat helped the armed underground. Had he consulted Zvi Bar-on and Dov Levin's *The Story of an Underground* (Jerusalem, 1962), among other sources, he would have realized that the Judenrat was far from compliant, that the Jews were misled by Soviet partisans into sending partisans to the Augustovo forests (which almost destroyed the underground), that Elkes tried to organize a mass escape from the ghetto, and so on.

Jewish armed resistance deserves better treatment and deeper analysis. Such an analysis would have to include a differentiated picture of Gentile attitudes, of objective and subjective difficulties, and of the way they were overcome in those instances where rebellious actions took place. Above all, armed resistance can hardly be taken out of the context of unarmed preparations, or unarmed resistance. The picture would have to include Slovakia, Yugoslavia, Hungary, and the West European countries to be complete. Ainsztein's book does not fill this need, despite the great effort and enormous good will that he has obviously invested.

YEHUDA BAUER
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high school (*Viša škola*). For a time he was the principal contributor to Svetozar Miletić's *Žastava*. Jovanović translated and published several important works, and he was a member of *Srpsko učeno društvo*. As a representative of Serbia's Liberals, he helped organize in 1866 the *Ujedinjena omladina srpska* (*Omladina*), a nationalistic youth organization influenced by the German *Burschenschaft* and *Tugendbund*, the Greek *Philike Hetairia*, and the Italian *Carbonari*.

The author discusses at considerable length the political activities of Jovanović, his position on basic issues of the time, and the organization and proceedings of the six *Omladina* congresses. He also compares Jovanović's philosophy and political tactics with those of Svetozar Marković. Until the appearance of Svetozar Marković on the scene in 1870, Jovanović was the principal ideologue of the *Omladina*. The book does not go beyond the 1870s when Jovanović became a member of the government and, in the eyes of many, compromised his principles.

More than an assessment of Jovanović's life and thought, this book is a study of the *Omladina* movement as a whole, and as such it contributes a great deal toward a better understanding of the political developments and tendencies in Serbian history during the third quarter of the nineteenth century. In fact, the author might have done an injustice to himself by focusing his study on Jovanović, who might not deserve the attention he receives, rather than on the *Omladina*, which definitely does. Also, the surprise exhibited over Westernizing tendencies in Serbia is not warranted. The important thing is, however, that the author has given us an excellent picture of a highly complicated period in Serbian history. His study is based on an extensive list of primary sources and secondary materials.

WAYNE S. VUCINICH
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GALE STOKES. *Legitimacy through Liberalism: Vladimir Jovanović and the Transformation of Serbian Politics*. (Publications on Russia and Eastern Europe of the Institute for Comparative and Foreign Area Studies, number 5.) Seattle: University of Washington Press, 1975. Pp. xvi, 279. \$11.00.

As the title of this book indicates, this is a study of liberalism in nineteenth-century Serbia through the philosophy and activities of one of its principal exponents, Vladimir Jovanović, who was among several young men sent abroad for education by the Serbian government in 1839. A politician, publisher, journalist, and scholar, Jovanović was also one of the first members of the Liberal party and an instructor in political economy at Belgrade's

JOZO TOMASEVICH. *War and Revolution in Yugoslavia, 1941–1945: The Chetniks*. Stanford: Stanford University Press, 1975. Pp. x, 508. \$20.00.

MATEO J. MILAZZO. *The Chetnik Movement & the Yugoslav Resistance*. Baltimore: Johns Hopkins University Press, 1975. Pp. ix, 208. \$12.00.

F. W. D. DEAKIN. *The Embattled Mountain*. New York: Oxford University Press, 1971. Pp. xiii, 284. \$9.50.

Interpreting the activities of the resistance movements in Yugoslavia in World War II, a many-sided civil as well as international struggle, was a matter of endless confusion and bitter controversy

at the time, not only among the embattled Yugoslavs but also for the outsiders who were playing for high stakes in this strategic piece of Balkan territory. History did not wait for historians to dispell the confusion and clarify the record; decisions were taken, campaigns fought, aid provided to one side or another. Tito's partisans won the civil war, while Mihailović and the Chetniks lost. But the legacy of the struggle remained, and the arguments over what had happened, and what might have happened, went on. They are likely to go on indefinitely, no matter how many sober scholars examine the evidence and present their reasoned conclusions. But it is a significant fact that, after thirty years, some competent and objective historians are doing just that.

Two American scholars, Jozo Tomasevich and Matteo Milazzo, have now given us admirable detailed studies of one of the parties in the civil war, the Chetniks. Working separately, they cover much the same ground and use many of the same sources, especially the extensive wartime records of the German and Italian occupation forces. (Although neither mentions the work of the other, they must have bumped into each other in the archives.) Milazzo's book is particularly good in describing the struggles taking place in areas annexed or occupied by Italy. Tomasevich's study, the first of a planned three-volume set that will cover the partisan movement and the quisling forces as well as the Chetniks, is broader in scope with background chapters on the prewar period and others on international diplomacy and the affairs of the Yugoslav government-in-exile.

On the issue that wracked all occupied nations, that of collaboration with the enemy, it became fairly clear during the war that the Chetniks were making deals in order to obtain arms (supplies dropped by the British were very meager), to carry on the fight with their domestic rivals, and to preserve their forces for the eventual rising when Western Allied armies would come to liberate Yugoslavia. Both books provide mountains of evidence that the collaboration was manifold, massive, and continuous; that Chetnik commanders made arrangements with the Germans, with the Italians, and even with the quisling Croatian regime in some instances; and that Mihailović himself, not just his lieutenants, deliberately chose that course. It flowed logically and almost inevitably from the fact that after the breakdown of the half-hearted efforts for Chetnik-Partisan cooperation in 1941, these rivals were in a war to the death to determine the country's future.

F. W. D. Deakin's *The Embattled Mountain* also covers the question of Chetnik collaboration. As one who was with the partisans through some of their most harrowing campaigns under heavy Ger-

man attack and who admired their fortitude and resolve, he could not be expected to be benevolent or neutral in his feelings toward the Chetniks, whose units were shooting at him and his comrades-in-arms. He was a keen and accurate observer, however, and his observations confirmed what earlier British missions (most of them sent in to make contact with Mihailović) had already concluded, though they had not always been able to get the word through to those responsible for high policy. Deakin's conclusions come through loud and clear, and his reports to his superiors and his extensive oral briefing of Winston Churchill in Cairo after returning from Yugoslavia in October 1943 had something to do with the decisive turn of British policy from Mihailović to Tito.

Deakin's book is a hybrid, as the author himself concedes. The first and last parts provide a gripping account of his own experiences when he was dropped by parachute in May 1943 into the cauldron of the fierce and bloody battle of the Sutjeska, and when he later took part in the events of the dramatic weeks following the Italian surrender. In between he describes the organization, policies, and personalities of the partisan movement, and he also reconstructs the story of the various British missions to the Yugoslav resistance groups. Here there is considerable new material, much of it based on conversations with Britons and Yugoslavs who were involved. The book is not the comprehensive study of the Yugoslav resistance that Deakin has declared his intention to write, but it remains a gem, a remarkable combination of personal memoir and historical inquiry written with spirit and style.

The story of the Chetniks, of course, did not end with the switch of Allied support to Tito in 1943. Mihailović was still hoping that events and possible American support would justify his strategy. He gained encouragement from the presence of an OSS mission under Colonel Robert McDowell, which remained with him until late 1944, long after the king and the government-in-exile, under British pressure, had dropped Mihailović and taken the road toward compromise with Tito. America's policy was not as clear as Britain's, but there was no real chance that Roosevelt would split with Churchill on Yugoslav policy by accepting the pro-Chetnik recommendations of McDowell. Tomasevich, who interviewed McDowell and looked at all the other evidence, covers this singular episode thoroughly and well.

It was the tragic destiny of Mihailović—a brave man, patriotic according to his own lights—that his base and his appeal were too narrow; he was essentially Serbian rather than Yugoslav in his loyalties. The Germans kept a firm grip on historic Serbia, his source of potential mass support, until

the Soviet army came in, and even at that late date he was still fighting the partisans instead of leading a rising against the Germans; and in the struggles waged in Bosnia, Croatia, and elsewhere the Serb population was decimated in Ustaši massacres and in the civil war (which was in large measure a war between Serbs). The Chetniks, as Tomasevich demonstrates again and again, could rally only a part of Yugoslavia's Serbs to their side and none of the other nationalities. Anticommunism was for them a genuine cause, but antifascism, the partisan watchword, was a stronger one for the bulk of the population. By fighting in the present, the partisans helped to shape the future, while the Chetniks made fatal compromises for a future that never came.

JOHN C. CAMPBELL
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ANDREW C. JANOS and WILLIAM B. SLOTTMAN, editors. *Revolution in Perspective: Essays on the Hungarian Soviet Republic of 1919*. (Russian and East European Studies.) Berkeley and Los Angeles: University of California Press, 1971. Pp. x, 185. \$10.00.

One of the essays in this volume is of classic stature. Written by Andrew Janos in an expressive English laced with Hungarianisms and carelessly proofread, the essay is based on extensive original research in Hungarian language sources. It undertakes, successfully in my opinion, the revision of the standard view of the history of *Ausgleich* Hungary as one of intransigent and feudal immobility. Rather, Janos argues, Hungary in the years 1867-1918 underwent a rapid, far-reaching, and centrally directed process of modernization.

The driving mechanism in Hungarian modernization consisted of two elements: a new state bureaucracy organized after 1867 and a political party (the Liberal) that without interruption managed to hold a majority in the lower house of parliament from 1875 to 1905. Both bureaucracy and party were made up primarily of persons of gentry origin. Through its domination of local government the bureaucracy controlled some 160 "rotten boroughs," located in the areas inhabited by the minority nationalities, so that the party had only to win some fifty seats in purely Hungarian districts, where elections were contested, in order to retain its majority.

Naturally, the party also looked after the interests of the bureaucracy, but the main concern of the new machine was that of national power. To this end the political machine rationalized the legal structure of the country, began the modernization of agriculture, improved the transportation net, encouraged industrial growth, and, above all, artificially depressed rural wages in order to provide for the accumulation of capital. Considering

the emergence of an industrial proletariat as a necessary evil, the gentry machine championed the emancipation of a rapidly increasing Jewish population, helping it become Hungary's first business class, in command alike of industry, banking, and commerce. The bureaucracy simultaneously swelled as a consequence of the effort to absorb the talented youth of the minority populations, which in this way were deprived of their own leadership; by 1918 roughly forty percent of ranking bureaucrats had non-Magyar names.

Modernization created its own opposition, however. The magnate class, which controlled fifty to seventy seats in the lower house and harbored reservations concerning both national state and market economy, attempted to undercut the new policy. A more far-reaching opposition developed among that portion of the ethnically Magyar gentry that could not be provided for in the administration. On the right, the gentry opposition stood for independence, the acquisition of a Balkan empire, and the achievement of European great-power status. On the left, the opposition gentry ended by collaborating with the Socialists, whose leadership in turn came from those elements of assimilated Jewry that could also find no place in the system. This prepared the way for the Károlyi *Gironde* of 1918-19. The workers, who provided the Socialist constituency, chose to concentrate on the fact that their living standards were low in comparison with those of Western Europe, rather than to remember that they were better off than other Hungarians. The peasantry, whose living standard under *Ausgleich* declined in absolute terms, was sufficiently affected by the growing volume of communication to express its simmering frustration in the bloody disorders of the Stormy Corner. Even the state bureaucracy found that its salaries had become the largest single item in the national budget. Modernization failed because the economic base was not broad enough to permit the Liberal machine to pay off all strategic groups.

The Janos essay takes up the first third of this tiny volume. Useful and competent chapters directly concerned with the Hungarian Soviet Republic are provided by Peter Kenez, Keith Hitchens, William B. Slottman, Richard Lowenthal, and Janos himself.

R. V. BURKS
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MARIUSZ KULCZYKOWSKI. *Andrychowski ośrodek płócienniczy w XVIII i XIX wieku* [The Textile Center of Andrychów in the 18th and 19th Centuries]. (Polska Akademia Nauk—Oddział w Krakowie. Prace Komisji Nauk Historycznych, number 31.) Cracow: Zakład Narodowy imienia Ossolińskich, Wydawnictwo Polskiej Akademii Nauk, 1972. Pp. 238. Zł. 45.

Kulczykowski's monograph is a study in the field of economic and social history. The author deals with the textile center that consists of seven villages surrounding the manor of Andrychow, which is located forty miles southwest of Cracow. The work chronologically spans the period from approximately 1750 to 1830, and it involves a population of twelve thousand at any given time.

In the introductory chapter the author offers a concise review of pertinent literature. He pieced his work together on the basis of information from archives in Warsaw, Cracow, and Lvov, which is now in the Soviet Union. He extracted a great deal of data from court books, from birth, baptismal, death, and matrimonial records, as well as from private collections and other less conspicuous sources. In this manner, Kulczykowski shows a new method of scholarly investigation. He also conducted a careful study of the topography of the area in terms of the drainage systems, the quality of soil, the type of forests—factors facilitating the production of linen.

The Andrychow weavers were peasants, bound to the manor by law, and were mostly illiterate. Kulczykowski shows that the whole social and economic life of the seven villages was centered upon the manufacture of linen. Having grown sufficient quantities of foodstuffs, the farmers cultivated flax, the staple raw material in linen production. Every household was in some way involved in this industry. The Andrychow textile center was unique in the author's opinion because the peasant-weavers controlled the entire apparatus of production, including the auxiliary crafts and the marketing of their product. This was in contrast to analogous centers in France, Prussia, and Russia, where outside factors intervened in the process. As regards marketing, the author proves, to his own amazement, that the practically illiterate peasant-mercers from Andrychow successfully vended their linen personally in distant places like Constantinople, Moscow, Warsaw, and the Balkans.

By reference to specific cases, Kulczykowski is able to bring out the real people of Andrychow: the small entrepreneurs, often indebted, with families living and working under the same roof. The thirty-three charts help the reader to understand the author's thesis, although illustrations of the equipment used by the weavers would also have proved useful. But to end on a positive note, the book illuminates the diverse relations between the peasants and the privileged nobility of Poland.

STANISLAW DABROWSKI
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EMANUEL HALICZ. *Partisan Warfare in 19th Century Poland: The Development of a Concept*. Translated by JANE FRASER. (Odense University Studies of His-

tory and Social Sciences, number 25.) Odense, Denmark: Odense University Press. 1975. Pp. 220. Dkr. 70.

After the partitions of their country, Polish patriots pondered over the dilemma of how a defenseless nation could recover its independence. What kind of an insurrection was feasible and most likely to succeed? Should it be directed against all the partitioning powers or solely against Russia, which held most Polish territories? Could final victory be won only through major battles or could guerrilla warfare also destroy the enemy?

In his interesting book, Emanuel Halicz explores nineteenth-century Polish military thinking together with its political and socioeconomic implications. The author, an emigré Polish historian, has written among others a monograph on the duchy of Warsaw and studies in military-political history. His present work makes a contribution to a field largely neglected by Western historians; given the relevance of partisan warfare for the present it might also interest nonspecialists.

Halicz provides his frame of reference in the introduction in which he also defines such terms as small-scale warfare, partisan warfare, and people's war. He proceeds then to a critical review of the polemical writings by Polish military theorists from the Kosciuszko-inspired pamphlet *Can the Poles Fight Their Way to Independence?* to the revolutionary vision of Kamieński. This indubitably rich body of thought has a utopian overtone, and one is tempted to suggest as a subtitle to the volume "from the myth of the scythe to the myth of people's war." Halicz ties Polish military thinking to Italian writings on partisan warfare and to general contemporary European experiences. Perhaps his most important and telling point is that advocacy of partisan warfare is not a satisfactory criterion for determining one's political ideology. Such a view is largely at odds with official present-day Polish historiography.

The translation is good, although there are occasional slips; perhaps the translator has stayed too close to the original Polish text. A nonspecialist may find the numerous details and the length of some of the quotations somewhat tedious. He will be gratified, however, by the good illustrations and maps. It is a pity that the book has no bibliography, but all in all it is a valuable study that enriches the meager body of Western-language publications concerning Poland in the nineteenth century.

PIOTR S. WANDYCZ
Yale University

JAN KOZIK. *Między reakcją a rewolucją: Studia z dziejów ukraińskiego ruchu narodowego w Galicji w*

latach 1848-1849 [Between Reaction and Revolution: A Study of the Ukrainian National Movement in Galicia in 1848-1849]. (Zeszyty naukowe Uniwersytetu Jagiellońskiego, 381, Prace historyczne, number 52.) Summary in English. Cracow: Uniwersytet Jagielloński. Pp. 236. Zł. 36.

There are technical and ideological hurdles confronting anyone writing on the complex events in Galicia in 1848-49. Indeed, the apparent specialized nature of the subject would militate against the need for a full discussion of the complexities of such labels as progressive and conservative, as well as a discussion of Engels' misjudgment of the events. Yet the role of the Ukrainians in Galicia remains important for understanding the vacillating policies of the Austrian government and the complex interaction of nationalism and socialism in Eastern Europe.

Jan Kozik clears the ideological hurdle by basing his analysis on his sources, not upon any preconceived predilections. He removes the technical hurdle, namely a paucity of sources, by his discovery of such primary materials as the long-lost minutes of the *Holovna Rus'ka Rada*, the major Ukrainian organization that emerged from the upheavals of 1848. He thereby renders a welcome service to students of the history of the area as well as of Ukrainian historiography. Moreover, Kozik makes full use of all other sources, including the contemporary press, which has not been readily available to earlier writers. Finally, he has made exhaustive use of secondary works and monographs, so that his book provides scholars not only with a discussion of the subject, but also with a full bibliography.

One can argue with Kozik's criticism of the pro-Austrian policies of the *Holovna Rus'ka Rada* and with his analysis of some of the policies of the Poles. He fails to point out that in Galicia "governmental policy" was often that of the local Polish administration and did not necessarily reflect the views of Vienna. These, however, are professional disagreements between specialists, which in no way detract from the overall excellence of the work.

The book is useful reading for anyone interested in the nationality policy of the Habsburg realm, in the Polish national movement, and in the formation of modern Ukrainian political and cultural organizations.

MARTHA BOHACHEVSKY-CHOMIAK
Manhattanville College

I. S. IAZHBOROVSKAIA. *Ideinoe razvitie pol'skogo revoliutsionnogo rabocheho dvizheniia (konets XIX-pervaia chetvert' XX v.)*. [The Ideological Development of the Polish Revolutionary Workers' Movement

(The End of the Nineteenth-Beginning of the Twentieth Centuries)]. Moscow: Izdatel'stvo "Nauka." 1973. Pp. 413. 1 r. 60 k.

This latest contribution of I. S. Iazhborovskaia to the historical literature on Polish Marxism is an exhaustively researched account of the development of the Polish Social Democratic movement from the founding of the party "Social Democracy of the Kingdom of Poland" (SDKP) in the spring of 1893 to the August 1923 Second Congress of the Communist Workers party of Poland, when a Polish party "of a new type" (Leninist) was established. In her study, the author draws heavily from the underground and exile press of the Polish movement as well as from documents of the SDKPiL and letters recently published in Poland and in the Soviet Union of prominent social democrats. In addition, Iazhborovskaia cites interesting portions of previously unpublished letters and notes of Feliks Dzierżyński, Cesaryna Wojnarowska, and Adolph Warszawski (Warski) located in the Central Party Archives of the Institute of Marxism-Leninism in Moscow.

The title of the book is misleading; the author does not discuss the revolutionary workers' movement. Instead, she concentrates on the leadership of the "left-wing" of Polish Social Democracy: the SDKPiL (the Lithuanian component was added in 1899), the PPS-left (founded in 1906), and Ludwik Kulczycki's PPS-"Proletariat" (1900-07). According to Iazhborovskaia, the political and ideological maturation of these parties depended on the proper understanding of the dynamics of the Polish working class, the "social-psychology" of the Polish "national-liberation movement," the vanguard position of the Russian revolutionary movement (the Bolsheviks), and the revolutionary potential of the Polish peasantry. Owing primarily to the distorting effects of the struggle against Pilsudski and the chauvinistic "social-patriotism" of the PPS, no Polish party sufficiently understood all the forces in Polish society. Despite their "short-sightedness" and "mistakes," Iazhborovskaia concludes, these parties still contributed to the progressively more "correct" (Leninist) application of Marxism to the problems of Polish class and national consciousness.

Nowhere in Soviet historiography is the cult of Lenin and its devastating effect on history writing so pervasive as in the literature on the Social Democratic movement. Iazhborovskaia's work is no exception, despite the valuable information she provides the Western reader. In an otherwise relatively balanced picture of the intellectual development of Rosa Luxemburg (especially on the national question), for example, the author misrepresents the important series of polemics be-

tween Luxemburg and Lenin on the organizational question. Their differences on this issue prior to 1905 are minimized. Luxemburg's famous critique of the Russian Revolution of 1917—the antibolshevism of which is over emphasized by both Western and Soviet historians—is simply omitted. Even Luxemburg's and Lenin's genuine friendship and the SDKPiL leader's enthusiastic response to bolshevism in the years immediately following the 1905 revolution are buried in the rhetoric of the cult of Lenin.

NORMAN M. NAIMARK
Boston University

JANUSZ JĘDRZEJEWICZ. *W służbie idei: Fragmenty pamiętnika i pism* [In the Service of an Idea: Memoirs and Writings]. London: Oficyna Poetów i Malarzy; distrib. by Piłsudski Institute of America, New York. 1972. Pp. 355. \$7.00.

The publication of a new primary source is always a significant event in the world of scholarship. These memoirs and writings by Janusz Jędrzejewicz (1885–1951)—the Polish minister of education from 1931 to 1934 and the premier in 1933–34—were compiled, edited, and annotated by the author's brother Waław, who succeeded him as minister of education and who was in his own right an important figure in Polish interwar politics. Part 1 consists mainly of the author's recollections of his early personal and professional life and of his public service in the Piłsudski government. For the most part they were written during his exile in Palestine after 1941 and his subsequent second exile in London. Part 2, a collection of Jędrzejewicz's addresses and speeches, provides a moral and theoretical explanation of Piłsudski's political philosophy. Though marked by a "great man" approach toward Piłsudski, Jędrzejewicz's account of the interwar political milieu in Poland is critical and frank. Jędrzejewicz emerges as a political moderate concerned with practical and concrete ends rather than with ideological or factional issues that dominated his associates.

The "idea" Jędrzejewicz served was, in the narrow sense, the wholesale restructuring of Polish education, which he carried out in 1932 and 1933. In the wider sense, Jędrzejewicz's "idea" was—as it was for Piłsudski—no less than the political, moral, and religious renovation of Polish life based on patriotism, loyalty, public service, and traditional principles. Jędrzejewicz's reforms created a uniform and logical multitrack system that reflected his personal conservative values and those of the regime. The political implications of his program, especially its elitist and statist character, drew opposition from both left and right extremists as well as from the Union of Teachers. The financial problems of the government in the

1930s reduced the effectiveness of Jędrzejewicz's impressive and ambitious plan.

Owing to their fragmentary nature, the memoirs are of uneven interest or value and do not yield a composite view of the period or the personalities. Contrary to my expectations, the section dealing with Jędrzejewicz's public role as minister and premier was not particularly informative. While it is always valuable to have a minister's own explanation of his policies, this account does not add substantially to existing information. The recollections pertaining to Jędrzejewicz's premiership are too incomplete to be of much use to scholars. The major interest of part 1 is Jędrzejewicz's dramatic account of the political maneuvering in Poland after the death of Piłsudski and his sensitive portrait of Walery Sławek. The close associate and heir apparent of Piłsudski, Sławek's influence waned after the death of the marshal; rebuffed and isolated, he ended his own life in 1939. Jędrzejewicz's profound understanding of Sławek's personality and political ideas, plus his insightful treatment of political machinations, contribute substantially to our appreciation of the internal turmoil in Poland on the eve of the Nazi invasion.

In addition to the fragmentary and uneven quality of the memoirs, other problems serve to limit their value and utility. The chronology in Jędrzejewicz's texts, for example, is not always easy to follow; the editor might well have supplied the relevant dates in brackets. Moreover, the silhouettes of personalities—with the exception of the profiles of Piłsudski, Sławek, and Śmigły-Rydz—are not as extensive or complete as the reader is led to believe. At times, Jędrzejewicz merely compiles anecdotes. As a whole, however, the memoirs are stylistically well written, astute, and forthright. Jędrzejewicz clearly explains his own positions and does not dissemble in appraising his associates in the Piłsudski government. Several points come through strongly in the memoirs: the economic and financial troubles of the period, the interplay between leftist and conservative ideologies, and the chronic factionalism that enervated the Polish Republic.

KENNETH F. LEWALSKI
Rhode Island College

EDWARD D. WYNOT, JR. *Polish Politics in Transition: The Camp of National Unity and the Struggle for Power, 1935–1939*. Athens: University of Georgia Press. 1974. Pp. xvi, 294. \$12.50.

When, after more than a hundred years of foreign rule, the Polish state was re-established in 1918, it was faced with a host of apparently insurmountable problems. The country was underdeveloped, the Great Depression further com-

pounded its economic problems, approximately one-third of the population of Poland consisted of national minorities, and, last but not least, it was faced by powerful and hostile neighbors both to the east and the west. By the late 1930s, with the international situation rapidly deteriorating in Europe, the external threat acquired alarming dimensions and at the same time the country found itself in the throes of a serious domestic political crisis.

After the death in 1935 of Marshal Piłsudski—the man who had played a key role in restoring Poland's independence and who had ruled the country as virtual dictator since 1926—political struggle developed simultaneously on two levels. First, there was the opposition to the continuation of the Piłsudskiites' dictatorship by political parties ranging from the extreme left to the extreme right. At the same time there was the internal rivalry, often no less bitter, among the various groups of Piłsudskiites themselves.

Edward Wynot's study outlines the activities of the different opposition parties and national minority groups, but it primarily describes the struggles among the Piłsudskiites. He traces step by step the rise of the group led by Marshal Śmigły-Ridz over the rival factions led by Colonel Ślawek and President Mościcki. Wynot describes the structure and activities of the Camp of National Unity, the mass political movement supporting Śmigły-Ridz, and he carefully analyzes the camp's unmistakable evolution toward totalitarianism.

Polish Politics in Transition is one of the best studies of the dramatic events in Poland in 1935–39. It is well researched, clearly organized, and carefully balanced in its judgments. As such it represents a valuable contribution to scholarship of interwar Polish history.

ADAM BROMKE
McMaster University

CHRISTOPH KLESSMAN. *Die Selbstbehauptung einer Nation: Nationalsozialistische Kulturpolitik und polnische Widerstandsbewegung im Generalgouvernement, 1939–1945*. (Studien zur modernen Geschichte, number 5.) [Düsseldorf:] Bertelsmann Universitätsverlag, 1971. Pp. 277. DM 34.

This study consists of two parts. The first and longer section is devoted to the conquerors; the second to the conquered. The first part illuminates Nazi intentions of enslaving the Poles. Culturally, the Nazi design was to deprive the Polish people of all but rudimentary education; like slaves, the Poles were left without cultural and esthetic assets and without an intellectual elite. Klessmann am-

ply demonstrates this thesis in his discussion of the Nazi-planned schools, the status of art during the occupation, the rigid control of the press by the German authorities, and the Nazi destruction of those institutions that had served to preserve the Polish heritage.

As the author shows, however, the Nazis were not wholly successful in implementing their designs. Competition and animosity among the leaders of the General Government and the civil, military, and SS authorities hindered many plans. Administrative chaos, changes in Nazi procedures and thinking, and the opposition of the Polish people also impeded the carrying out of the Nazi blueprint.

The last part of this volume focuses upon three elements among the Polish underground: the educational structure, the press, and the literati. Pre-1918 conspiratorial considerations guided the illegal schools. Polish educators in the underground—many of them leaning to the left—attempted to achieve during World War II what the Old Regime had denied them: the democratization of education. Academia acted under diverse conditions, and the almost semilegal institutions paired with the secret ones. At the same time, writing, in the press and elsewhere, followed strict partisan lines and conveyed ideas along the entire political spectrum. Although Janus-faced, the original, thought-provoking output was somehow meager, as Klessmann indicates.

This scholarly work, balancing description with analysis, evokes associations, and the author alludes to a few. He describes the tradition of resistance to oppression among the Polish people, and he notes the differences between the residents of Warsaw, who were more experienced in conspiracy, and the residents of Cracow, who were relatively unseasoned in underground activities. (No doubt, a more systematic comparison of Poland's sections from the times of partition would be enlightening.) Furthermore, Klessmann records the similarity of the Polish and Czech experiences under the Nazis (he could add the Slovenes' as well), but unfortunately without elaboration. Poland contained, too, a ready-made testing group, the Jews, although Klessmann only points to the frequency with which the Polish Jewish community was studied. Yet, one feels compelled to ask what caused the Polish Jews to react in so similar a fashion to the Polish Gentiles in their resistance (including the cultural and intellectual aspects) to their mutual captors.

Klessmann's scholarship is a welcome addition to the literature on wartime Poland and on anti-Nazi resistance.

YESHAYAHU JELINEK
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ANN M. KLEIMOLA. *Justice in Medieval Russia: Muscovite Judgment Charters (Pravye Gramoty) of the Fifteenth and Sixteenth Centuries*. (Transactions of the American Philosophical Society, New Series, volume 65, part 6.) Philadelphia: the Society. 1975. Pp. 93. \$5.00.

This monograph, a revised Ph.D. dissertation produced in Horace Dewey's workshop on late medieval-early modern Russian law at the University of Michigan, is based on a meticulous and definitive analysis of about 156 published judgment charters. These documents, which summarize the cases, were awarded to the victorious litigants to show that they had won court suits, should the matter be contested in the future. (About 30 of these judgment charters are translated with extensive commentary as part of Dewey's and Kleimola's *Russian Private Law. XIV-XVII Centuries. An Anthology of Documents* [1973].)

Ann Kleimola, currently at the University of Nebraska, is to be congratulated for her mastery of the Middle Russian legal and vernacular language of these documents. More interestingly, she has progressed beyond the philological stage to write a first-class piece of history, one that uses current secondary materials. The judgment charters show the legal life of Muscovy in the fifteenth century, when justice had become triadic (the court supervised the process, but did not run an inquisition; the litigants themselves were responsible for making a case move along). Written records were replacing the earlier oral stage, but the state had not yet developed adequate record-keeping facilities to retain all verdicts. When the state bureaucracy reached that level around the beginning of the seventeenth century, according to Kleimola it ceased issuing judgment charters.

Using each judgment charter an average of six times while analyzing all aspects of Muscovite justice they portray (officialdom; litigants and their claims; trial procedures—testimony of witnesses, documentary evidence, duels and oaths; and instances), the author concludes that there was a high degree of legality in Muscovy. Decisions were rational, predictable, according to written law and custom, and even intelligible to Americans four centuries later. Justice was not necessarily class-oriented, so that a peasant could win in litigation with a prince. Corruption was at most of minor significance. The work, which also shows the high degree of Muscovite centralization, is a major contribution that will be of interest to students of legal, social, and Russian history.

RICHARD HELLIE
University of Chicago

R. G. SKRYNNIKOV. *Perepiska Groznogo i Kurbskogo: paradoksy Edvarda Kinana* [The Correspondence of

Groznyi and Kurbskii: The Paradoxes of Edward Keenan]. (Akademii Nauk SSSR, Institut Russkoi Literatury.) Leningrad: Izdatel'stvo "Nauka." 1973. Pp. 136. 83 k.

The correspondence between Tsar Ivan IV Groznyi of Russia and Prince A. M. Kurbskii, and Kurbskii's history of Ivan's reign, have been considered the most original literature and important historical documents of sixteenth-century Moscow. Kurbskii was Ivan's military commander in the Livonian War. He defected to Lithuania in 1564 after which he purportedly wrote Ivan from Wolmar, Livonia. By 1579, he allegedly wrote four more times, and Ivan replied to him twice. Although there has never been a critical edition of the correspondence or of the "history," they have appeared in many editions and translations and have been cited extensively by historians. It is not, then, surprising that when Edward Keenan suggested that the entire correspondence and the "history" were forgeries (*The Kurbskii-Groznyi Apocrypha* [1971]), it raised a scholarly furor. (See the extended reviews of C. Halperin, A. Kappeler, and I. Auerbach, *Jahrbücher für Geschichte Osteuropas*, 22 [1974]: 161-213.) Keenan's discovery that a passage of the "Complaint" of the Lithuanian Orthodox monk Isaiah dated to 1566 had been copied into the Kurbskii letter of 1564 was the starting point of his argument against authenticity. Keenan further argued that Prince C. I. Shakhovskoi in the 1620s was progenitor of the correspondence. He then traced a logical textual development of additions and later editions throughout the seventeenth century.

R. G. Skrynnikov, the author of three books and many important articles on Ivan IV, now has written a sweeping rejection of Keenan's thesis which reaffirms the traditional dating and authorship. In the process Skrynnikov offers fresh evidence and analysis of the manuscript tradition of the correspondence and of related documents, of the conditions under which Kurbskii wrote the first letter and Ivan his long answer, of the likelihood of Shakhovskoi's authorship, and of the content and language of the letters. He does this against a framework of contemporary and later sources on the reign of Ivan IV (Kappeler, and G. Orchard, *Canadian-American Slavic Studies*, 16 [1974]: 448-59, discuss Skrynnikov's arguments in detail). Skrynnikov's reconstruction of the manuscript tradition differs fundamentally from that of Keenan and is more convincing. His reconstruction of the events of Kurbskii's defection and of the "Wolmar convoy" of letters, within which Kurbskii's first letter appeared, also is persuasive. The new textual evidence includes analysis of the manuscript containing Isaiah's "Complaint" by which Skrynnikov

shows that the "Complaint" probably was written before 1564. Skrynnikov also makes a case for Isaiah's links to Orthodox emigrés in Wolmar to explain how Kurbskii obtained the document. Finally, Skrynnikov shows that the content of the correspondence correlates well with other sixteenth-century literature. Especially important is his discovery of a reference to letters of Kurbskii and Ivan in the "Opis' tsarskogo arkhiva," a document of unquestioned sixteenth-century origin. Much in Skrynnikov's analysis remains problematical, but the case he makes is strong. The burden of proof remains with Keenan and others who might continue to question the authenticity of the correspondence, and it is a heavy burden indeed.

DAVID B. MILLER
Roosevelt University

O. N. VILKOV, editor. *Goroda Sibiri (Ekonomika, upravlenie, i kul'tura gorodov Sibiri v dosovetskii period)* [The Cities of Siberia (The Economy, Management, and Culture of Siberian Cities in the Pre-Soviet Period)]. (Akademiia Nauk SSSR, Sibirskoe Otdelenie, Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka." 1974. Pp. 297.

G. P. BASHARIN. *Nekotorye voprosy istoriografii vkhozhdeniia Sibiri v sostav Rossii* [Some Questions on the Historiography of Siberia's Entry into the Russian State]. Iakutsk: Iakutskoe Knizhnoe Izdatel'stvo. 1971. Pp. 134.

A. N. KOPYLOV. *Ocherki kul'turnoi zhizni Sibiri XVIII-nachale XIX v.* [Essays on the Cultural Life of Siberia from the 17th to the Beginning of the 19th Century]. (Akademiia Nauk SSSR, Sibirskoe Otdelenie, Institut Istorii, Filologii i Filosofii.) Novosibirsk: Izdatel'stvo "Nauka." 1974. Pp. 251.

Siberia's historical relationship to European Russia remains an important issue in Soviet historiography. These three works on Siberia's "feudal" past (1580-1850) agree upon the ultimately positive, progressive results of Russian conquest. Otherwise, they have little in common. Narrow in focus and strictly factual is the anthology, *The Cities of Siberia*, containing fourteen relatively unrelated, vignette-style essays. They discuss aspects of social life, architecture, industry, and administration in selected towns or areas. The reader must pose broader questions, such as: What did these towns have in common? What military role did they originally play, with their defense forts and paramilitary personnel (*voevody, sluzhilye liudi*, and Cossacks)? When and how did industrial capitalism and a bourgeoisie emerge? The editor, however, making no claims to overall synthesis, merely dedicates the book "to the important, but weakly elaborated problem of the Siberian town in our historiography."

Kopylov's work, *Essays on the Cultural Life of Siberia*, is of major historiographical interest. The author presents excellent surveys of education, architecture, painting, and theatrical entertainment (literature is excluded). Regarding Siberian culture as "part of overall Russian culture," he points out common developments as well as regional variants from the Russian norm. More a province than a colony of Russian culture, Siberia was in step with developments in European Russia in the areas of literacy, technical and public education, town planning, and theater. However, in the introduction of baroque and classical architectural styles and in the emergence of secular painting and artists, Siberia experienced a cultural lag.

State, church, and eventually urban public initiative comprised the most active forces after 1700 in advancing cultural innovations, usually along Western lines. State motivations were chiefly educational (to acquire a body of literate, trained officials at the local level) and often arbitrary. Without a gentry, in Siberia the church played a larger role in education, art, and theater than it did in Russia proper. Also atypical were the contributions of exiles and scientific expeditions, especially in painting. Kopylov concludes that the "official ruling culture" was more influenced by a "democratic" input in Siberia than Russia. Although he asserts that native Siberian ornamentation entered into art and architecture, neither text nor illustrations substantiate this. Claims of a distinct "Siberian baroque" style are likewise unconvincing. Despite minor flaws, Kopylov's study is exemplary in its research, organization, and thoroughness.

Broadest in conception, and least satisfying of the three works, is Basharin's *Some Questions on the Historiography of Siberia's Entry into the Russian State*. The book lacks integration and amounts to an exercise in "history as it should have been" and "should have been written." Historiography is used as a base for lambasting "falsifiers" of Siberian history (at home and abroad, recent and past) and for interspersing indictments of Nazi, Japanese, and Western imperialism. Following the majority line among Soviet historians, Basharin staunchly contends that Iakutia, Siberia, and other outlying areas entered "voluntarily" and peacefully into the Russian territorial complex—and not by conquest or forced annexation. In contrast to Kopylov and the anthology contributors, Basharin stresses "correct" historiography and opinion, giving little attention to the documentary record.

Of the three works, Basharin's raises the most provocative questions concerning the nature of any territorial absorption, the relationship between central authority and new territory, attitudes of

masses and elites, natives and settlers. Analogies with the American "winning of the West" often come to mind. Though all three studies unconditionally paint tsarism in dark colors, the state itself is shown as administrative unifier, cultural pacesetter, defender of Russian territory, and upholder of "Russia's historical mission" (Basharin, p. 70).

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S. E. TOLYBEKOV. *Kochevoe obshchestvo kazakhov v XVII-nachale XX veka: Politiko-ekonomicheskii analiz* [The Nomadic Society of Kazakhs in the Seventeenth to the Beginning of the Twentieth Century: A Political-Economic Analysis]. Alma Ata: Izdatel'stvo "Nauka," Kazakhskoi SSR. 1971. Pp. 632.

This study of the Kazakhs in the three centuries before the 1917 Revolution serves three purposes for Western historians: first, since it is a substantially revised and enlarged version of a 1959 book published by Tolybekov, *Obshchestvenno-ekonomicheskii stroi Kazakhov v XVII-XIX vekakh*, it shows the professional development over a decade of the "dean" of Central Asian historians; second, it provides an inside view of the state of Soviet historical and sociological science; and, third, it gives the most current interpretation held by Soviet historians of the effect of annexation and revolution upon the Kazakh people. It is sad that a fourth purpose, providing important historical and sociological knowledge about the Kazakhs, is not one of Tolybekov's goals.

More than twice as long as his first effort, the book under review shows both methodological and substantive maturation but almost no development of a conceptual nature. While Tolybekov earlier had been content to base his more surprising statements about Kazakh society on a few carefully selected texts of Marx and Lenin, in this volume the author takes great pains to expand his source material. He has examined archival materials, has read many more prerevolutionary authors, and, save for the notable lack of any non-Russian or non-Soviet accounts, has used virtually all relevant materials. Yet the introductions and conclusions to every topic considered are exactly the same as before. Not a single supposition or conclusion has changed. This is a remarkable feat in an area subject to such intense debate and about which so much has been written in the last decade. Tolybekov has expended all of his efforts at providing more documentation of both a theoretical and substantive nature for his earlier conclusions.

The work is representative of the type of problem that Soviet historians and sociologists are

studying. It concentrates on the benefits of Russian and Soviet modernization on the Kazakhs rather than on the results of modernization in a traditional society. Theoretical and methodological input is aimed at the justification, not analysis, of events in Kazakhstan. Tolybekov provides evidence of a considerable amount of debate within Soviet scholarship on this topic. Harshly criticizing the works of A. Erenov and I. Zlatkin, who wrote on feudal relationships among the Kazakhs, Tolybekov shows that there is not unanimity on the conclusions drawn from the evidence, just unanimity on the correct topics and approaches to pursue.

Finally, the reader learns that not only did the Kazakh people greatly benefit from having joined the Russian state, but that the unification was the result of a natural development. The Kazakhs before the mid-nineteenth century did not have a society, a state, or a culture of their own. They "could not" have had these, for their society was a good example of a "semi-nomadic" construction that, in Tolybekov's view, exists in a no man's land between a feudal nomadic society and a patriarchal settled one. The rulers of such a society draw their power from the nomadic base; the people exist in a settled condition. Thus the semi-nomadic society contains such social and political inner contradictions that it is meaningless to discuss state or culture in one. An example of the extent to which Tolybekov is willing to go to prove his case is that in his first book he included a Chinese fourth-century poem, purportedly from nomadic portions of the state. In his second book he repeats this poem (p. 52) but deletes two lines that speak of "motherland." Here he uses this to support his case for the nomadic Kazakhs' lack of thought on nation or homeland.

In his introduction Tolybekov states that his task is "using Marxist-Leninist methods, to clarify the main questions of the socio-economic structure of the nomadic economy of the Kazakhs . . . , to analyze the facts of the history of the Kazakh people, and to examine the benefits to the Kazakh people of their annexation to Russia." A task that includes the third goal must of necessity ignore the second. Tolybekov does.

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DAVID L. RANSEL. *The Politics of Catherine's Russia: The Panin Party*. New Haven: Yale University Press. 1975. Pp. x, 327. \$17.50.

David L. Ransel's monograph is an important addition to the growing list of revisionist writings on the reign of Catherine II. Its subject is the relationship between Catherine's government and one of

the networks of clan ties and patronage that united and divided her statesmen. Ransel sees these networks, which he calls "parties," as being critically important to the workings of Catherine's government, for it was through them that the monarch ruled, first aligning herself with one group, then controlling and counterbalancing it, and eventually replacing it with another more suited to her changing tastes and purposes.

Within this general framework Ransel focuses his attention on the "party" led by Nikita Panin. Refuting Sacke and others who saw in Catherine's statements and actions of the 1760s the results of a struggle between the empress and an aristocratic opposition led by Panin, Ransel shows that the Panin group was in fact predominant throughout most of that decade and collaborated enthusiastically and loyally with Catherine. Identifying good government with themselves and their own political views, the members of Panin's group developed their criticisms of Catherine's regime in the 1770s as they lost influence first to the Orlovs and then to Potemkin, until finally Nikita Panin himself was dismissed in 1781. Their critique of the Potemkin era in the 1770s and '80s established the fundamental position for those who would view Catherine negatively, but it was misunderstood and misused both by her successor, the Emperor Paul, and by historians when they misapplied that critique to the whole of her reign, including the early years, when the Panin group itself had been in favor.

The great problem for both the author and the readers of this book is the absence of any work as good as Ransel's on Teplov, Viazemskii, the Orlovs, the Chernyshevs, and others. Because of that, Ransel has trouble fitting some of the participants into the bipolar system of politics he envisions. At one point he describes Viazemskii as "a man attached to neither of the leading parties," but elsewhere calls him Orlov's "creature." Similarly, although he is vague on the political relationship of Teplov and Shakhovskoi to Panin, Ransel uses some of their actions and writings (but not others) to indicate Panin's position on certain issues. Ultimately, Ransel's focus on Nikita Panin blurs and distorts his view of other statesmen, including Catherine herself.

Nevertheless, Ransel's book is more convincing than any of its predecessors in depicting the politics of Catherine's court, and it contributes significantly to our understanding of her reign.

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FRANCIS LEY. *Alexandre I^{er} et sa Sainte-Alliance (1811-1825)*. Preface by PIERRE PASCAL. Postscript

by GEORGES BIDAUT. Paris: Librairie Fischbacher. 1975. Pp. 328.

The title of this excellent study is precise. Beginning with a careful account of the "*cheminement spirituel*" of Tsar Alexander I under the impact of various religious and mystical groups, Francis Ley traces in great detail the fortunes of that strangest of diplomatic documents, the Treaty of Holy Alliance, from its drafting by the Tsar in 1815 to his death in 1825. In a concluding chapter, "L'Agonie de la Sainte-Alliance," Ley shows how Alexander's genuine hopes for a new age in diplomacy and religion dissolved under the conservative pressures of Metternich abroad and of reactionary leaders within Russia.

When this reviewer undertook some forty years ago a study of Madame de Krüdener—the name generally associated with Alexander and the Holy Alliance—he soon found that many important documents used, albeit imprecisely, by the Genevan Charles Eynard in his basic two-volume biography (1849) were no longer to be found. In 1959, through the well-known Paris bookseller, Raymond Clavreuil, he met Francis Ley, a Paris banker and a direct fifth-generation descendant of Madame de Krüdener. In the salon of the Ley villa at Asnières were the missing documents, a veritable "Krüdener archive." Using this material Ley published his *Madame de Krüdener et son temps* (1961), a work crowned by the French Academy. Now, with other unpublished material and an exhaustive use of the printed sources in French, German, English, and Russian, Ley gives us a splendidly written and meticulously documented account of this unusual chapter in European history. His bibliography is indispensable.

Ley has not attempted to give a full diplomatic history of the period. He keeps Alexander in the central position, demonstrating the impact upon him of a large number of foreign figures such as Jung-Stilling, Franz von Baader, the Herrnhuters, and the English Quakers, as well as Russians such as Golitsyn, Kochelev, and Labzine. By 1815, all this had become a veritable bombardment directed, one must feel, to a man already convinced that he was acting under the immediate guidance of God. Yet with the revolutionary outbreaks of 1820, Alexander came to associate some of the "mystics" with a new revolutionary temper which he abhorred. Hence he turned against them and died a tragic figure.

Ley's story, in outline not new, has a solidity which earlier accounts lacked. He has done for Alexander what the psychohistorians might wish to have done, but he has worked by means of massive documentation and legitimate interpretation. It is difficult to see how this important

contribution to religious, political, and social history could have been better presented.

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J. L. BLACK. *Nicholas Karamzin and Russian Society in the Nineteenth Century: A Study in Russian Political and Historical Thought*. Toronto: University of Toronto Press. 1975. Pp. xvi, 264. \$17.50.

The study of Russian historians in the English-speaking world has been surprisingly neglected, especially in view of their importance for shaping views on the evolution of the Russian state and society. J. L. Black's book is a welcome addition to the limited Western analysis of Russian historians.

The book is not narrowly historiographical and considers Karamzin in the broad context of Russian political and historical thought. The author's major purpose is to demonstrate the "social and political nature" of the contribution made by Karamzin's writings to the image held by Russians of themselves and of their past. In light of this goal, the best single chapter is chapter 5 ("The *History* and Russian Society in the Nineteenth Century"), for here the author is most concrete in specifying the contribution of Karamzin's writings. Textbooks, historical novels, scholars' lectures and publications, and the salon conversations of the literati reflected the wide distribution, use, and impact of the *History*.

Given its singular success with the Russian reading public, the *History* became significant as a symbol of Russian nationalism. This subsequently led to the development of "Karamzinism," wherein Karamzin became an icon to his supporters and was identified with political and social views occasionally not his own. Black's view of Karamzin is essentially that of an eighteenth-century figure committed to the rationalizing effect of enlightenment for both ruler and ruled, but who reacted in a patriotically conservative manner to influences and events growing out of the French Revolution. Eventually Karamzin's *History* became an obstacle to the further development of Russian historical scholarship, owing to its use by both liberals and conservatives as a touchstone for conservative views on Russia and its political future.

Black successfully explicates Karamzin's historical themes and illustrates the extent of their acceptance, but his achievement stimulates the wish that it might be possible to delineate the extent and depth of Karamzin's persuasiveness in nineteenth-century Russian society with greater precision. Perhaps as Western and Soviet scholars devote more attention to the study of nineteenth-century Russian historians, there will be less likeli-

hood of judging that these matters are "incalculable," as Black assumes.

Based on printed sources, the book contains a selective bibliography emphasizing recent books and articles. Published with care in an attractive format, the book should be in all good collections on Russian history in both undergraduate and graduate libraries.

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RICHARD G. ROBBINS, JR. *Famine in Russia, 1891-1892: The Imperial Government Responds to a Crisis*. (Studies of the Russian Institute, Columbia University.) New York: Columbia University Press. 1975. Pp. xiv, 262. \$12.50.

Richard G. Robbins has discovered a compact body of archival material, especially in the papers of the Economic Department of the Ministry of Interior and the Special Committee on Famine Relief, which chronicles the Russian government's reaction to the great famine of 1891-92, and which details its efforts at famine relief. He has worked through these previously unstudied papers carefully to produce a model historical monograph. True, the topic itself is narrowly focused. But it is meticulously researched, the narrative is carefully constructed, and the conclusions are solidly based.

It is the administrative question—the efforts of the tsarist government to deal with the scourge of famine in the late nineteenth century—which concerns Robbins. A discussion of the manner in which state policies evolved and were implemented in response to the famine of 1891-92 constitutes the major portion of his work. The author's careful analysis of documents in Soviet archives effectively disputes many of the myths which have emerged during the past three-quarters of a century concerning the manner in which the imperial government dealt with the famine. Certainly, he shows clearly that the Russian statesmen were deeply concerned with the famine, and that their efforts to meet the crisis can hardly be characterized as incompetent or completely ineffectual. "The [Russian] government succeeded in mounting one of the largest relief campaigns in Russian history" (p. 168). Robbins writes, and his detailed discussion of the government's efforts on that score more than proves his point.

This immensity of the Russian government's relief campaign cannot be doubted; more to the point in assessing its impact is the question of its effectiveness. Certainly, as Robbins indicates, the efforts of the tsar and his advisers were not without shortcomings. More important, however, Robbins proves that despite "serious institutional and political obstacles, . . . government assistance averted

the very real threat of mass starvation, held the death rate within acceptable limits, and prevented a total economic collapse in the stricken region" (p. 173). Yet despite the government's considerable success in dealing with the immediate crisis, the famine of 1891-92 was a critical watershed in Russia's history. The debate over the famine had politicized Russian life, and, as Robbins argues, "too many questions had been posed by the famine; too many aspirations had been raised by the regime's own programs for combatting the crisis" (p. 182). The result, he maintains, was that "the government's attempt to return Russia to the calm it had known during the *zatisa* of the 1880s was doomed to fail" (p. 182).

In conclusion, Robbins' study considerably broadens our knowledge of how the imperial government functioned at the end of the nineteenth century. Equally important, his study adds to our understanding of how absolutist (and underdeveloped) states seek to deal with grave social and economic crises.

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BERND BONWETSCH. *Kriegsallianz und Wirtschaftsinteressen: Russland in den Wirtschaftsplänen Englands und Frankreichs, 1914-1917*. (Studien zur modernen Geschichte, number 10.) [Gütersloh:] Bertelsmann Universitätsverlag. 1973. Pp. 256. DM 29.

In this published dissertation, Bernd Bonwetsch contributes substantially to our understanding of British and French economic goals vis-à-vis their erstwhile Russian ally during World War I. In the process the author also helps, though not explicitly, to put the issue of German war aims into a comparative perspective. There was, Bonwetsch makes clear, no lack of Anglo-French ambitions toward Russia. Both powers had designs for markets, raw materials, and commercial advantages; what was lacking was an ability to translate those goals into reality.

The study's conclusions are quickly stated. Britain and France wanted to replace Germany as the major supplier of goods to Russia. British private interests pressed for concessions but with inconsistent government support; in France, by contrast, the government led the effort to secure long-term arrangements, especially postwar supplies of raw materials and preferential tariff treatment. In these endeavors London and Paris were driven by a realization that the American colossus might soon pre-empt their opportunity to supplant Germany. Despite minor concessions, obviously derived from the war situation, neither Britain nor France succeeded in its schemes. Their failure stemmed in part from Russia's own determination

to control its postwar economic destiny, in part from the progressive deterioration of the Anglo-French economic position as the war continued.

These findings suggest a Russian government with far more freedom of action than either Soviet historians or the late G. F. Hallgarten would concede to the tsarist regime. Yet Bonwetsch's case would have been helped, and certainly made clearer, had he indicated the total amount of economic exchanges—including war loans, the sale of strategic goods, and deferred payments—that took place among the Triple Entente countries. For background on these matters, René Girault's *Emprunts russes et investissements français en Russie, 1887-1914* (Paris, 1973) is indispensable. Further, Bonwetsch would have buttressed his position had he consulted more private collections, especially those of senior British officials. And the entire work would have benefited from a conclusion that actually concluded. These caveats notwithstanding, the author has provided a useful guide to a hitherto *terra incognita*—inter-Allied relations during World War I, a terrain where it was almost as dangerous to be a friend as a foe.

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NORMAN STONE. *The Eastern Front, 1914-1917*. New York: Charles Scribner's Sons. 1976. Pp. 348. \$15.00.

Norman Stone argues in this book that the Russian defeat in the First World War cannot be explained by economic backwardness alone. He shows that the shortage of munitions, while real, was exaggerated by some generals to cover up their own mistakes. The main cause of the shell shortage of 1915 was that the Russians, just like everyone else, had expected a short war. By 1916 they had managed to make up the deficiencies. According to Stone the country suffered not so much from the lack of economic development per se as from the feverish attempts to overcome it. Modernization exacerbated the social tensions that ultimately brought down the empire. This thesis is well taken and a useful corrective to those who would accept uncausal, simplistic explanations of Russia's defeat.

The great value of the book, however, is not its novel thesis but the author's ability to present military history. Stone knows a great deal about strategy and tactics; he is familiar with questions of military organization and has an impressively thorough knowledge of the armies of the Central Powers. As a result, he is able to put the Russian effort in comparative perspective. In his best chapter he describes the Brusilov offensive and is able

to pinpoint the achievements of the Russian general as well as the errors of the Austrians.

In spite of its virtues this is an extremely irritating book. Stone is a historian in the tradition of A. J. P. Taylor: he believes his main task is to debunk legends and disprove fabrications. (He uses the words "legend" and "fabrication" with distressing frequency.) Obviously it is useful to question commonly accepted generalizations, but the Taylor approach has two dangers: the historian is tempted to set up straw men in order to knock them down, and in being overanxious to contradict prevailing notions he may cease to be judicious. In this book Stone often makes both errors.

For example, there is no need to "disprove the legend" that Russian agriculture at the time of the First World War was dominated by a few large estates. No serious historian holds such a view. Similarly the author is knocking on open doors when he essays to prove that agriculture was not damaged by the war. Michael T. Florinsky, in his important book, *The End of the Russian Empire* (1931), made the same point. (This title, incidentally, does not appear in Stone's thin and unsatisfactory bibliography.)

The author usually champions the unconventional point of view: he takes Falkenhayn's side against Ludendorff's, and Sukhomlinov's against everyone else. He believes that the *zemgor* and the war industry committees made only a small contribution to the war effort and that the Russian army did not fall apart in 1917. These judgments are suspect because Stone obviously does not have the same firm grasp of Russian history as he has of military matters. For instance, it is wrong to say that "Lenin could lay some claim to patrician status" when in fact his grandfather was a serf. It is untrue that a poor man could not attend the Academy of the General Staff because of the high cost, for not only was the academy free, but the officers received a salary while attending it. It is far too simple to divide the prewar officer corps into two groups: patrician and "preaetorian" (whatever that means). The confident assertions that "the Red Army in 1918 established a partnership with the people and therefore there was a rush of volunteers to join it" and that "the Bolsheviks attracted what was the best in the Russian officer corps" are simply false. If Stone was correct it would be hard to understand why the Reds in the spring of 1918 could not suppress the White Volunteer Army that had only three thousand participants. The author's belief that the tsars wanted to use the peasants against their masters betrays great ignorance of Russian social history.

This book should be read, but with caution.

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LEOPOLD H. HAIMSON, editor. *The Mensheviks: From the Revolution of 1917 to the Second World War*. Translated by GERTRUDE VAKAR. (Hoover Institution Publications, number 117.) Chicago: University of Chicago Press. 1975. Pp. xxiii, 476. \$22.50.

This volume is the result of seminars that involved surviving Mensheviks and some American and European scholars. The five Mensheviks, who contributed essays based on documentary evidence and recollections, concentrated only on events personally known to them. The four-part product is not a comprehensive history of menshevism from 1917 to 1939, nor even a chronologically or thematically consistent volume. The greater part of the book concerns the pre-Stalinist Soviet period with only slight coverage of the party in emigration. The work is quite valuable for its bibliographical information, for its factual detail, and, most importantly, for what it reveals about the Menshevik movement and its influence on twentieth-century Russia.

Though the essayists maintain that objective circumstances more than ideology dictated Menshevik policy in 1917, the book clearly reveals the Mensheviks' blind commitment to doctrine thereafter. Most important was the theory of the bourgeois character of the Russian Revolution—the primacy of economics, which according to their laws of historical development made the collapse of the Bolshevik regime inevitable. Some Mensheviks even saw the New Economic Policy as a confirmation of the traditional concept of bourgeois revolution, even though the NEP witnessed an onslaught of systematic persecution of the party and by 1923 saw the Mensheviks driven underground. As for 1917, the theory of bourgeois revolution was slightly hazy on the question of coalition, which bedeviled the Mensheviks between March and November. The Menshevik Geneva Conference of April–May 1905 had breached the theory with the point about the "partial and episodic" participation of Social Democracy in governmental organs created by the revolution (hardly unequivocal prohibition as some scholars write). Therefore, though justified in their skepticism concerning the primacy of theory in 1917, Leo Lande and Boris Sapir do not buttress their argument with an analysis of the period between March and June 1917 to demonstrate that menshevism indeed formulated policy on the basis of objective circumstances and not according to theory. The essayists' failure to treat this crucial period in the history of menshevism is a major weakness of the book. However, the memoirs of I. G. Tseretelli reveal that he, as the pervading force in menshevism's apogee, tried to have his cake and eat it, too; that is, Tseretelli and his associates tried to prevent civil war by joining a coalition as individuals, not

as leaders of their party (at best an imperfect compromise between circumstances and theory).

While circumstances and theory shaped the Menshevik position on state power, the consequences of their views, as democrats, derived entirely from theory. The Menshevik F. I. Dan has noted that the paradox of socialism versus democracy led menshevism to emphasize democracy more than socialism after 1905, while bolshevism sought socialism at the expense of democracy. As these essays reveal, not only did their democratic commitment limit their field of action, but their ability to act as well. As democrats, they relied solely on the art of persuasion and agitation to accomplish their goals—shunning any suggestion of coercion. The essayists show that the Mensheviks decided after November 1917 to combat the Bolsheviks—but peacefully. According to these essays, however, circumstances, not theory, decided the Mensheviks' fate. The Menshevik strategy of an anti-Bolshevik sympathy among the workers failed because the party could not foresee Lenin's surrender to Germany, the willingness of Imperial Germany to recognize and support his regime, and the terrorism that was to outrage the world. Menshevik strategy was not confined to snatching slogans from the Bolsheviks but of snatching the Bolsheviks out of Lenin's hands. Even when menshevism finally proved unequal to the contest, the movement's saving grace was its commitment to democracy and human decency that did not degenerate into totalitarianism, of which Lenin was the architect.

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ROMAN KUPCHINSKY, compiler. *Natsional'nyi vopros v SSSR: Sbornik dokumentov* [The Nationalities Question in the USSR: A Collection of Documents]. (Obshchestvenno-Politicheskaya Biblioteka, number 42. Dokumenty, number 13.) Munich: Suchasnist. 1975. Pp. 440. Cloth \$9.50, paper \$7.95.

This anthology constitutes the first attempt to compile recent materials, which are devoted exclusively to the nationalities question, originating in the Soviet Union, the majority of which fall into the category of *samizdat*. The first six chapters deal with the Ukrainians, the Baltic nations (Latvians, Estonians, and Lithuanians), Crimean Tatars, Belorussians, Jews, and Georgians; the final chapter includes documents on the Armenians, Meskhethians, and Germans, as well as two collective declarations in defense of national rights signed by representatives of the Baltic states, the Ukraine, and the Caucasian region.

The largest single group of documents deals with the Ukrainian national movement and includes, among others, such important materials as

the recommendations of the Republican Conference on Problems of the Culture of the Ukrainian Language (1963) to party and state authorities, the petitions and works of the well-known dissidents Valentyn Moroz, Sviatoslav Karavansky, and Ivan Dziuba, and Mykhailo Braichevsky's historical essay "Annexation or Reunification?" Documentation of the national movements of the Jews, Crimean Tatars, and the Baltic nations is not as well represented, and I feel that although the anthology is quite bulky (440 pp.), in view of their significance, additional materials relating to these groups should have been included. This is particularly true in the Jewish case. Finally, another aspect of the nationalities question in the USSR that might well have been taken into consideration in the preparation of such a volume is the position of various Russian nationalist (and in some cases chauvinist) and neo-Slavophile groups vis-à-vis the non-Russian nationalities.

It is clear that Kupchinsky and the Suchasnist publishers are to be congratulated for bringing us such a fine collection of documents on the current Soviet scene. In view of the continually growing Western interest in the nationalities question in the USSR, it is hoped that we shall soon see an English-language edition of this useful and important anthology.

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GEORGE ALEXANDER LENSEN. *The Damned Inheritance: The Soviet Union and the Manchurian Crises, 1924-1935*. Tallahassee, Fla.: Diplomatic Press. 1974. Pp. xi, 333. \$19.80.

The inheritance to which the title of this book refers is the Chinese Eastern Railway, with its diplomatic, strategic, and economic importance. It was bequeathed to the Soviet Union by Tsarist Russia. For various reasons the Soviet Union found it inadvisable to act on the spirit of the Karakhan Manifesto and surrender its rights to the railroad. As a result, it was obliged to defend them by force against a Chinese challenge in 1929 and then sell them, under duress, to Japan in 1935.

Lensen's account is based primarily on the foreign relations documents recently published by the Soviet Ministry of Foreign Affairs, the Japanese Foreign Ministry's secret *History of Negotiations between Japan and the Soviet Union* (declassified and republished in 1969), and British Foreign Office archives opened in the mid-1960s. His major thesis is that the brief Russian sally into Manchuria was a strictly limited operation in response to extreme provocation and that the Soviet Union's policies after 1931 amounted to an orderly retreat in the face of a Japanese takeover, which it was powerless to prevent. Such a nonaggressive policy, Lensen

argues, should dispose of the contention that Japanese expansion in Manchuria was to some extent justified by the Soviet menace, for no real threat existed. (As a corollary Lensen points out that the Russian incursion in 1929 was so different from the Japanese conquest of Manchuria that failure of the powers to support China in the former instance can scarcely be judged the fatal precedent that led to the Pacific War.)

While the corollary seems reasonable, the major thesis is less compelling. Diplomats' reports and exchanges provide too narrow a range of evidence for an understanding of the reasons why a given policy was pursued. Russian military weakness vis-à-vis Japan in Manchuria in 1931 is no index of the strategic threat that she might have been expected to pose within a few years. Japanese perceptions of and attitudes toward Russia need to be integrated into an overall analysis of Japanese foreign policy in a more systematic fashion than is attempted in this study, particularly in view of the extreme tension between the two countries in 1931-34 that Lensen documents (not to mention Soviet efforts in Mongolia and China proper). It should also be noted that Lensen's treatment of the Russo-Chinese dispute of 1929, while providing a useful corrective to the anti-Russian interpretation, does not do full justice to the Chinese.

Still, the diplomatic record of these controversies remains an indispensable part of the story, and Lensen has provided an extremely valuable and detailed introduction to it.

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JOHN ERICKSON. *The Road to Stalingrad*. Volume 1, *Stalin's War with Germany*. New York: Harper & Row. 1975. Pp. x, 594. \$25.00.

In 1962, John Erickson wrote *The Soviet High Command: A Military-Political History, 1918-1941*. Thirteen years later, despite the appearance of much new material and the publication of several similar studies, his book remains the definitive work on the subject and is likely to remain so. *The Road to Stalingrad*, although not quite up to the same Olympian standards, is destined to receive the same recognition.

In view of the sheer size of the project, it might have been better to follow the Russian example of devoting one volume to each year of the war. Nonetheless, Erickson has written the best narrative to date of the first seventeen months of the greatest armed struggle in history. He traces in minute detail the history of Hitler's preparation for operation "Barbarossa," the paralysis and confusion of the Soviet government and military in the early

days of the war, the Soviet counterattacks, the devastating defeats in the summer of 1942, and, finally, the first phase of the Battle of Stalingrad. He presents several possible reasons for the unpreparedness of the Red Army in 1941 and, even more perplexing, for the failure of Soviet intelligence and armed forces in the summer of 1942. He tends to blame Soviet military doctrine for these catastrophic defeats, and he shows that Stalin was a meddlesome bully and not at all an exceptional military leader. His views on the effect of the purges on the performance of the Red Army, however, are ambiguous and even contradictory (pp. 6-7, 20).

Most writers depend primarily on Western sources; Erickson has an encyclopedic knowledge of Soviet sources as well, and he uses them with great discretion. His preface to the study is an excellent guide on how to use Soviet sources and is essential reading for those who want to make use of Soviet historical writings. It is inexplicable why he does not use General Grigorenko's *Der sowjetische Zusammenbruch*. This is the harshest criticism of Stalin's military leadership, in which Grigorenko persuasively argues that in 1941 the Red Army was both quantitatively and qualitatively superior to the Wehrmacht. Also, in his references Erickson omits a fair number of important sources, such as the 1969 edition of *Kievskii Krasnoznamennyi*, which remains the most detailed Soviet description of the campaign on the southwest front. Despite these omissions, his extensive use of Soviet sources is one of the strongest points of the book. It is doubtful that any significant Soviet material is going to be released in the near future. Since the ascendancy of the Brezhnev-Kosygin regime, omission and falsification as well as glorification of Stalin's role have been the rule in writings about the military events of 1940-42.

The book is not without faults. Erickson has decided to dispense with footnotes and instead to rely on a bibliographical essay for each chapter, a practice that I find unsatisfactory. There are no maps, not even copies of the excellent Russian ones, and there are no tables of command and lists of units, information that is easily obtainable. There are a fair number of minor factual errors, giving the book the appearance of a "rush" job. Finally, the index, obviously done by the publisher, leaves something to be desired.

These criticisms should in no way detract from the monumental work of Erickson, who remains our foremost scholar of Russian military history. We can be sure that if better military histories of the USSR are written, then Erickson will write them.

MICHAEL PARRISH
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Z. BASIN'SKII *et al.*, editors. *Dokumenty i materialy po istorii sovetsko-pol'skikh otnoshenii*. Volume 7, 1939-1943 gg. Moscow: Izdatel'stvo "Nauka," 1973. Pp. 509.

As might have been expected of an official documentary collection edited jointly by Soviet and Polish historians and dealing with the stormy relationship of the two governments during World War II, the present volume does not tell us much about this relationship that we did not already know. More than two-thirds of the 304 documents printed in the volume have already been published elsewhere, while of the new ones, only a half dozen or so add significantly to our knowledge. (Among the new documents the ratio of Soviet to Polish sources is on the order of six to one.)

If the new collection is compared with that published by the Polish exiles in London, which covers approximately the same period (*Documents on Polish-Soviet Relations*, vol. 1, 1939-43 [London, 1961]), important gaps emerge. Basin'skii and the other editors provide no documents concerning the German-Soviet negotiations pursuant to the fourth partition of Poland in 1939. They permit only indirect reference to the forcible deportation from the newly annexed territories of up to 1.5 million Polish citizens. The repeated inquiries of the London government as to the whereabouts of the 8,300 Polish officers taken prisoner by the Red Army in 1939—namely, questions raised prior to the discovery of approximately 5,000 bodies in the Katyn Forest during April 1943—find no place in this book. Even the only conversation between Sikorski and Stalin (on December 3, 1941) does not appear in this volume, evidently because Sikorski asked about his officers.

Furthermore, the editors regularly suppress evidence of Soviet unwillingness or inability to supply the Polish divisions, formed in the USSR from among refugees and former prisoners, with Soviet standard equipment and rations; of the substantial number of Poles that Anders was not permitted to recruit; and of the persistent efforts of the London government to secure the evacuation of elements of the refugee population. Such lacunae must reflect more accurately the tensions that currently underlie Soviet-Polish relations than they do the events of World War II.

R. V. BURKS
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O. B. BORISOV and B. T. KOLOSKOV. *Soviet-Chinese Relations, 1945-1970*. Edited with an introduction by VLADIMIR PETROV. Bloomington: Indiana University Press, 1975. Pp. xviii, 364. \$12.50.

Writing under pseudonyms in order not to impair their future dealings with the Chinese, two high-

ranking Soviet specialists on China have penned a comprehensive account of Sino-Soviet relations since the occupation of Manchuria by the USSR in the final stages of the Pacific War to 1970 (the book was published in the Soviet Union in 1971), as perceived through Russian eyes. They detail the assistance provided by the USSR to the Chinese Communists (1945-49) and the People's Republic of China (1949-52), the cooperation between the two neighboring powers (1953-59), the gradual deterioration in Sino-Soviet relations in the 1960s, and the anti-Soviet character of the Cultural Revolution. They ascribe the roots of the conflict to Mao's "cult of personality," his opposition to "peaceful coexistence" with the United States, his ingratitude for Russian assistance, and his unwillingness to coordinate his policies with Moscow.

Unlike many American authors who depict the split as a manifestation of historical enmity between Russia and China, the Soviet writers portray it as an aberration, produced by one faction in Peking. In presenting the policy of Moscow in a most benevolent light and blaming Maoist chauvinists for the Sino-Soviet dispute, they add new grist to the mill of controversy. At the same time, however, they stress that the conflict is neither desirable nor natural and that the Soviet Union stands prepared to terminate it.

The frustration that the Russians have experienced in their relations with the Chinese is genuine. It was shared at one time or another by Japanese, English, and American supporters of various Chinese governments. These powers were likewise alienated by the lack of expected gratitude on the part of the Chinese for the aid extended to them. Deep as the rift between Peking and Moscow may be, as recorded in the book and proclaimed by the present Chinese leadership, it would be perilous for American policy makers to count on its indefinite continuation.

The book is an important contribution to the history of Sino-Soviet relations. It was translated by computer and verified by David Chavchavadze. The translation was heavily edited by Vladimir Petrov of the Institute for Sino-Soviet Studies of George Washington University (who also supplied an introductory essay) and polished (with redundancies deleted) by Sally G. Bunting. The result is a tightly knit, readable account, more easily digested than the Russian original.

GEORGE ALEXANDER IENSEN
Florida State University

GEORGE LENCZOWSKI. *Soviet Advances in the Middle East*. (U.S. Interests in the Middle East Series. Foreign Affairs Study 2.) Washington: American Enterprise Institute for Public Policy Research, 1972. Pp. 176. \$4.00.

The Arab defeat in June 1967, followed by the protracted Israeli presence in the Arab lands, brought about a massive deterioration of the United States position in the Arab orbit. With the sharp decline of American political influence in the late 1960s and early 1970s came the loss of economic predominance, particularly in most of the Arab oil-producing states. These developments prompted the American Enterprise Institute to initiate a series of studies on American interests in the Middle East, under the editorship of George Lenczowski. The volume under review, written by Lenczowski, is the seventh in this series. It is devoted to the general theme of the recent progressive Soviet penetration of the Middle East.

After a brief introduction setting forth the doctrinal underpinnings of Soviet foreign policy, the author carefully analyzes the historic Soviet quest for expansion in the key states of the region—Iran, Turkey, Egypt, and Iraq. The chapter on Iran examines briefly the turbulent phase of Soviet-Iranian relations during the 1940s and early 1950s, with particular reference to oil concessions and the Azerbaijan problem. Lenczowski follows this with an account of the shift of Soviet policy from unsuccessful subversion to the establishment of normal economic relationships during the 1960s. The chapter on Turkey contains an excellent review of Soviet-Turkish relations during and after World War I, including the Moscow-Ankara rapprochement and the consequent destruction of the Armenian Republic, the Straits question, and the Kurdish problem. The author analyzes the growing dissonance in Soviet-Turkish relations as these concerned Soviet strategic and territorial demands after World War II. He devotes the final section to Turkey's NATO role, the Cyprus issue, and the normalization of Soviet relations with Turkey.

Lenczowski's most ambitious chapters focus on the expanding Soviet role in Arab lands. He identifies the factors facilitating Soviet penetration: the rise of Arab nationalism, the establishment of Israel, and the Western imperial presence. He describes the methods used to implement it—arms deliveries, cultural offensives, technical assistance, and economic aid. The convergence of Soviet and Arab interests is detailed, focusing on the development of close ties between the USSR and Nasserite, radical regimes espousing pan-Arabism and Arab socialism. The chapter on Egypt contains valuable sections on the role of Egyptian leftist groups and the evolution of Soviet economic and political influence until the early 1970s.

Nowhere in the Arab world was indigenous communism stronger than in Iraq. The author points out that the Iraqi Communist party was unique in attempting to capture power in an Arab country. Coupled with the deeply factionalized

state of Iraqi society, this produced a "checkered pattern" of relations with the Soviet Union. Indeed, so unstable and paranoic had been the Iraqi regime that systematic Soviet attempts to cultivate in Iraq a reliable ally were repeatedly aborted.

The concluding chapters include a detailed estimate of the Soviet military contribution to Arab armed forces and the consequent Soviet strategic "presence" in the area. Despite the general success of Soviet efforts, the author identifies certain obstacles operating in the Arab areas. These include the uncompromising nature of Arab nationalism, which defied Soviet attempts to exercise greater political control, and the deep admiration of most Arabs toward the West and its superior technology. Lenczowski concludes by returning to his main theme that the Arab-Israeli conflict is primarily responsible for the decline of the position of the United States among the Arabs—one filled by the Soviets.

The chief weakness of this competent analysis is that it is dated because of the rapid changes in Middle Eastern politics.

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I. M. MUMINOV *et al.* *Samarkand Tarikhi* [History of Samarkand]. In two volumes. Tashkent: Izdatel'stvo "Uzbekiston SSR FAN Nashrieti." 1971. Pp. 474; 481.

This two-volume work in both Russian- and Uzbek-language editions is a useful addition to the growing body of materials concerning the local and regional histories of the Soviet Union. Samarkand and its vicinity, long an important area of western Central Asia, are presented in a multifaceted manner from earliest times to the present with discussions of social, economic, cultural, and political history. The general theme of the work is the relationship of all the peoples of Samarkand and its environs, regardless of their ethnicity, to a deep local heritage rather than to a broader Islamic or general Turkic-Iranian background. It stresses that this local heritage and potential after many long centuries finally achieved full development in the Soviet period.

The first volume, especially in its treatment of the ancient and medieval periods, places substantial reliance on the works of Barthold and Vambery. These sources are supplemented by materials from native histories and the results of Soviet period archeological work. Both volumes feature lengthy bibliographies of great utility to the student and scholar in the West. The first volume has the sources conveniently divided into those from the Soviet and pre-Soviet periods. The

first volume contains little new information on political developments, but the inclusion of social, cultural, and economic history into the same work adds a new dimension. Emphasis is on the Islamic period, with most attention given to the Timurid era. Of somewhat unique interest are three chapters discussing the development of irrigation systems in this river valley and oasis area during the ancient, medieval, and early modern periods. The second volume, dealing with the Soviet period, emphasizes social and economic themes. It is perhaps the more valuable of the two for the student and scholar, because it draws heavily on local materials, both published and unpublished, that are for the most part unavailable outside the Soviet Union.

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NEAR EAST

J. G. MACQUEEN. *The Hittites and Their Contemporaries in Asia Minor*. (Ancient People and Places, volume 83.) Boulder, Colo.: Westview Press. 1976. Pp. 206. \$18.00.

As volume 83 in the useful Ancient Peoples and Places series, this book presents a nontechnical, up-to-date introduction to the history and culture of the Hittites. Footnotes are sparse, but a select bibliography and sixty-one line drawings of plans and objects provide a measure of documentation. The first third of the book is a sketch of the history of the Hittites and their neighbors in Anatolia during the second half of the second millennium B.C. J. G. Macqueen states his method of historical reconstruction: "Events fit into a general pattern of economic necessity, and the efforts of the various states can be seen to be directed towards something more permanent than the arbitrary ambitions of individual sovereigns" (p. 41). For the author the determining factor in the political history of Anatolia was the presence of large deposits of copper ore (at Ergani Maden, for example), which were carefully secured, exploited, and traded in distant quarters of the ancient world. Since tin was not available in Anatolia, the routes by which it was brought to metalworking centers were jealously guarded. This intriguing thesis lacks only the support of archeological evidence for metalworking installations and slag heaps. One wishes for more detailed documentation for this far-reaching interpretation of the course of political events in Anatolian history than has been possible within the limits of a volume of this series.

The latter two-thirds of the book deal with topics of cultural interest: daily life, warfare and

defense, society, religion, art, and literature. This section follows the rubric of the synthesis made by Götze in 1933, but it fills in the picture by using the wealth of new material that has appeared since. Macqueen summarizes his story of the Hittites as follows: "Their rediscovery has revealed to us a people who, if they did not possess the genius and originality to change the course of world history, at least showed a talent for political and military organization, and a capacity to utilize their resources, which enabled them to gain, and retain for several hundred years, a leading position in the Middle Eastern world" (p. 150). Although work in progress may change details in the story, it is not likely to alter materially this appraisal.

JAMES B. PRITCHARD
University of Pennsylvania

LEWIS V. THOMAS. *A Study of Naima*. Edited by NORMAN ITZKOWITZ. (New York University Studies in Near Eastern Civilization, number 4.) New York: New York University Press. 1972. Pp. xii. 163. \$12.50.

Mustafa Naima was an official historian of the Ottoman Empire. His history, composed between 1697 and 1704, covers events during the reigns of eight sultans (Ibrahim is curiously omitted from the list on page 128) from A.H. 1000 (1591-92) to A.H. 1070 (1659-60). Thomas' study, based on Naima's history, is also a study of Naima himself. In a broader sense, it is a study of various Ottoman institutions and attitudes of the seventeenth and eighteenth centuries.

One will not find here a précis of Naima's chronicle or a critique of its adequacy or accuracy as history. Instead, Thomas has written about Naima's life, about his ideas and attitudes, especially as expressed in the two prefaces of his work, and about his use of sources and his critical methods. The whole book is in fact a sort of Ottoman commentary on aspects of Ottoman life and thought. The fact that Naima was born in Aleppo of a Janissary father leads Thomas, for example, to introduce sections of explanation on Aleppo officialdom and its relations with Arab tribes and on Janissary units in a provincial station. His study of Naima's prefaces leads Thomas to comment on Naima's ideas on the virtues of a strong ruler, on advice to viziers, on factions, and so forth. The commentary is often brilliant. The text and notes are crammed with information, including occasional translated passages from Naima's text.

Lewis Thomas, before his untimely death in 1965, was America's leading teacher of Ottoman history. This study was his doctoral dissertation. A former student, Norman Itzkowitz, is the editor.

It is good to have the study, already influential in manuscript, finally in print.

RODERIC H. DAVISON
George Washington University

ILKAY SUNAR. *State and Society in the Politics of Turkey's Development*. (Ankara University Faculty of Political Science Publication, number 377.) Ankara: Ankara Üniversitesi Basımevi. 1974. Pp. ix, 196.

This stimulating Berkeley dissertation examines the linkage between the traditional Ottoman political structure, the state, and the emergence of a new entity, the civil society, during the last days of the empire.

In the first chapter İlkey Sunar points to the unique nature of the Ottoman Empire and the inappropriateness of likening it to medieval Europe, insofar as the former was based on patrimonial rule and the latter on a clear-cut distinction between state and society inherent in the feudal order.

Chapter 2, in the discussion of the political and economic state of the Ottoman Empire, offers the reader an intelligent analysis of some of the deleterious forces at work that militated against the economic development of the empire; for example, the close integration of the empire with Europe's economic and political structures; the patrimonial nature of the empire resulting in a regulated rather than a free economy; the tensions between the Ottoman state and nascent society "created" by bureaucrats; the emergence of patronage as a mechanism for economic development as an adjunct to the patrimonial order; and so forth. For the historian, this detailed chapter is the most rewarding. The third chapter is devoted to political events since 1923; as such, it covers familiar ground.

Of great interest to the author is the role of the middle class in the Republic of Turkey, a topic that occupies the lengthy fourth chapter. To be sure, Sunar argues, there has been economic development since 1923, but it has been lopsided, limited, and of a questionable nature owing to the differential distribution of the fruits of this development.

Analysis of events in Turkey since its founding as a republic leads the author to conclude in his final chapter that what is now taking place in Turkey is a "politics of mobilization," that is, the urban proletariat and the peasantry have made an effort to change the situation so that they, too, will be the beneficiaries of economic growth.

This well-written and well-researched monograph is full of stimulating insights and observations. It is hoped that Sunar will, however, further

develop some of his seminal theses and theories more fully in later publications.

This work successfully integrates the disciplines of history, sociology, political science, economics, and public administration in forcefully expounding the author's perspectives. Historians might wish that Sunar had devoted a larger portion of his study to history. However, this is a minor criticism of an otherwise outstanding work.

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DICKRAN H. BOYAJIAN. *Armenia: The Case for a Forgotten Genocide*. Westwood, N. J.: Educational Book Crafters. 1972. Pp. xiii, 498. \$15.00.

The focus of this volume is the extermination of over half of the Armenian people by Ottoman Turkey during World War I. As a volunteer in the French Army fighting against the Turks, the author witnessed the final phase of the deportations and atrocities that befell the Armenians. Yet his book is not an eye-witness account; rather, it is a historical-legal brief presenting the Armenian case in quest of justice. Indeed, Boyajian's methodology is that of a trained lawyer—which he is—rather than the craft of a historian. He is basically motivated by an Armenian's deep sense of outrage not only against Turkey but against the world for having forgotten the plight of his people. Six decades after the loss of two million souls, the Armenians remain uncompensated materially or territorially despite the promises made by the Allies during and after the Great War. Thus the author's stated purpose is dual: to awaken the conscience of the world and to present the historic case for the establishment of an "Armenian homeland" similar to the Jewish state of Israel.

The first chapter documents the governmental planning behind the massacres by presenting the translations of several official telegraphic communications that contain specific orders of deportation and extermination. In chapter 2 the author details the progressive decline of the Armenians' position in the Ottoman Empire, including the massacres of 1895-96 and 1909 as seen through the eyes of American and European missionaries, diplomatic officials, and travelers. This is followed by the weakest part of the book—an account of 3,000 years of Armenian history. Next, the story of the massacres of 1915 is presented in some detail by drawing from official German, French, British, and American sources. Significantly, few Germans, except Count Wolff-Metternich, attempted to stop the Armenian massacres, despite their pervasive military presence in the territory of their Ottoman ally. Ironically, it was a Jewish-American, Ambassador

Morgenthau, who unsuccessfully pressed the Young Turkish leaders to stop the atrocities.

The remaining portions of the book deal with the aftermath of the holocaust, including the desperate efforts of the surviving Armenians to set up a homeland under an American mandate and the failure of the Allies to help sustain the Armenian Republic, which eventually collapsed in 1920 in the face of the developing Soviet-Turkish rapprochement. A massive appendix—consisting of additional documentation on the massacres, statements by world leaders concerning the Armenian question, as well as relevant treaties, conventions, and maps—ends the volume.

Clearly, Boyajian's study is the result of a prodigious amount of work, and despite its unorthodox, legalistic format it represents a strong validation of Armenia's tragedy. His use of the Jewish holocaust as a point of departure is enlightening and apparently sound, although a comprehensive analysis of comparative genocide has not yet been attempted. Nor can Boyajian's volume be considered the definitive work on the Armenian massacres; indeed, such a major effort cannot be undertaken as long as the relevant sections of the Ottoman Archives of Istanbul remain closed to foreign scholars.

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HARRY N. HOWARD. *Turkey, the Straits and U.S. Policy*. Baltimore: Johns Hopkins University Press. Published in cooperation with the Middle East Institute. 1975. Pp. xii, 337. \$14.50.

Almost half a century ago Harry N. Howard began to write about Turkish-American relations. While pursuing careers in the State Department and as a university professor he published an impressive series of articles and monographs. More than half of this new book deals with the period before World War II when the United States was politically disinterested in the Middle East, except for the World War I interlude. Howard's account, spanning the period from the first Turkish-American treaty in 1830 until 1939, is useful, especially for the detail it provides on the Straits Conventions of Lausanne (1923) and Montreux (1936). But Howard and others have already covered this ground well.

The chapters dealing with the Straits issue since 1939 are more important. Howard's long account of World War II is more than an examination of the Straits question; it provides interesting detail on Turkey's nonbelligerent role, as well as Grand Alliance discussions on possible Turkish entrance into the conflict. Cold war historians will find Howard's analysis of Stalin's role interesting;

while officially favoring Turkey's active participation in the war, Stalin was much more interested in the establishment of a second front and possible Turkish concessions to the Soviet Union in the Straits and elsewhere. These objectives would be more difficult to pursue after the war if Turkey assumed belligerent status.

Howard makes clear why a Soviet-Turkish crisis after the war was almost inevitable. Pursuing its historic objectives in the Straits area from a more powerful base, the Soviet Union in 1945 and 1946 demanded substantial revision of the Montreux Convention—to include Soviet bases in the area and joint Turkish-Russian administration of the strategic waterway. The United States and England backed Turkey's nervous but resolute determination to withstand Soviet pressure. As Howard points out, Truman chose to get tough on this issue. Truman told Secretary of State James Byrnes that "unless Russia is faced with an iron fist and strong language another war is in the making." The promulgation of the Truman Doctrine in 1947 further illustrated the president's commitment to Turkey.

This study is effectively documented with original and secondary materials. A useful appendix provides texts of several key documents, comparison of various Straits conventions, and statistical tables on commercial shipping in the Straits since World War I. An annotated bibliography is also provided. Howard's important work is a fitting climax to the career of a pioneer scholar of United States-Middle Eastern relations. Another book on the subject of the Straits will be unnecessary for many years.

ROGER R. TRASK
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R. J. GAVIN. *Aden under British Rule, 1839-1967*. New York: Barnes and Noble. 1975. Pp. x, 472. \$35.00.

LAWRENCE R. PRATT. *East of Malta, West of Suez: Britain's Mediterranean Crisis, 1936-1939*. (Published in cooperation with the London School of Economics and Political Science.) New York: Cambridge University Press. 1975. Pp. xiii, 215. \$17.95.

In his introduction to *The View From Steamer Point* (1964) Charles Johnston stated that R. J. Gavin's full-length study of the British in Aden would soon be in print. Now, eleven years later, *Aden under British Rule, 1939-1967* appears, the first work detailing British presence in Aden and the hinterland—the Yemen and the Hahdramawt—during 128 years of British rule. At present the capital and main port of the People's Republic of Southern Yemen, Aden was until 1967 the most important part of England's life line to India. This study

describes British policy-makers' activities pertaining to Aden and their relations with sultans, sheiks, emirs—friends, allies, and enemies—and it describes an evolving economy in what was first a refueling port and then a free port and center for a land and sea-borne trade, which included coffee, gums, hides, skins, and incense from the shores of South Arabia and the Red Sea. The latter part of the book traces the development of Aden's modern industrial society, including treatment of the formation of trade unions; the growth of educational, medical, and social welfare institutions; for formation of the Aden Federation; the revolution in Yemen and the nationalist struggle to take over Aden; and the ultimate British withdrawal.

Gavin explains Aden's external affairs as they related to Egypt. With the opening of the Suez Canal in 1869, however, foreign affairs begin to be slighted in the text, but at times Italian aspirations and "meddlings" come to the fore. An important segment of the latter, with its emphasis on the Mediterranean, can be read in Lawrence R. Pratt's *East of Malta, West of Suez*, an analytical and interpretive study. Its theme is an explanation of the crisis involving Mussolini that came at the time of the Italian Abyssinian invasion and enveloped an overextended empire, insufficient revenue, and inability to defend the empire. The crisis was magnified by the weakness of the League of Nations, Hitler's machinations, the Spanish Civil War, French insecurity, the lack of policy in American foreign affairs, and the inability of British statesmen to agree on how to contain Mussolini because of his unpredictability or "mad-dog tactics." Since policy centering on the Mediterranean cannot be separated from Europe, the Balkans, or even the Far East, Pratt delineates a second theme, appeasement, and exonerates Hoare in the original negotiations with Laval and Chamberlain, the two scapegoats for most statesmen. Gavin suggests that appeasement provided valuable time for reinforcement of defenses at Singapore, for building additional ships, and for rearmament at home.

Based on extensive primary sources, Gavin's and Pratt's studies show competency and clarity of thought. Gavin, in his bibliography, misspells Charles Johnston's name, and he makes no mention of Johnston's participation in the Aden Federation. Pratt superficially places his topic in its historical setting and pays too much attention to appeasement. Both authors present excellent psychological portraits of the men involved, discussing their dealings, shortcomings, and loyalties. The importance of the Mediterranean, the Red Sea, and Aden to Britain and the empire is stressed. In his conclusion Gavin explains how the British hurriedly left Aden, thus symbolically ending the final chapter on the British life line to India. Pratt

also indicates in his work that English indecision in the Mediterranean from 1936 to 1939 only foretold the reality of the situation.

KATHLEEN E. DUNLOP
East Carolina University

BEN ZION WACHOLDER. *Eupolemus: A Study of Judaeo-Greek Literature*. (Monographs of the Hebrew Union College, number 3.) Cincinnati: Hebrew Union College Press, for the Jewish Institute of Religion; distrib. by Ktav Publishing House, New York. 1974. Pp. xi, 332. \$15.00.

Eupolemus is reputed to be the first Graeco-Jewish historian known to us. There are a number of problems, however, attendant to this second-century B.C. diplomat and member of a Jerusalemite priestly family. His work survives only in fragments preserved in the writings of the Church fathers, Eusebius and Clement of Alexandria, whose source was Alexander Polyhistor. Complicating the matter is that part of Eupolemus' history is said by some, including Ben Zion Wacholder, to have been written by a historian they call Pseudo-Eupolemus, identified as a Samaritan.

The six surviving fragments of Eupolemus' history cover the period from Moses to his own day. Wacholder sees Eupolemus' history as a continuation of the work of the Biblical Chronicler who allowed himself complete freedom in elaborating Jewish history to indicate the great value of Judaism and to enhance the position of certain Jewish heroes. Thus, in the first fragment we read that "Moses was the first wise man, the first who imparted the alphabet to the Jews; the Phoenicians received it from the Jews, and the Greeks from the Phoenicians; also laws were first written by Moses for the Jews."

Eupolemus' goal apparently was to promote Judaism for those Jews who were profoundly influenced by Greek culture by indicating that Jewish culture was the oldest in the world and that it was Jews who really taught others, rather than vice versa. Somewhat later, Philo of Alexandria attempted to do the same by noting that Plato learned from the Bible and the teachings of Moses. What we have then, Wacholder concludes, is an essentially new history of the Jews, fusing biblical with Hellenistic historiography, in response to the needs of a generation torn between the attractions of Hellenism and their loyalty to Judaism.

While much of Wacholder's analysis is influenced by the work of J. Frudenthal, his major contributions are the detailed linguistic analysis of the fragments and the use he makes of them in exploring the nature of Graeco-Jewish literature which flourished in Palestine in the second century B.C. This literary tradition, seen as following the

style of the Book of Chronicles, continued in the histories of Josephus and Justus of Tiberias. This is an impressive study of problematic material, offering important insights into a period of great cultural flux.

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EDGAR O'BALLANCE. *Arab Guerilla Power, 1967-1972*. Hamden, Conn.: Archon Books. 1973. Pp. 246. \$10.50.

That old phenomenon, irregular warfare, and the much more recent, but still old, word "guerilla" [*sic*] seem to have taken on a strange fascination in recent years. Many appear to believe that the phenomenon itself did not occur before the emergence of recent theories of "people's war." Palestinian Arabs have engaged in some fairly impressive acts of this kind, in 1936-39 especially, and in 1950-56; but only the post-1967 actions have attracted much attention. O'Ballance follows the majority. His theme is the adoption of Maoist theory by the Palestinians and their fortunes in its application. The book relates how a variety of underground Palestinian organizations, most notably al-Fath (O'Ballance uses the more common "Fatah") were formed after 1956 and gained popularity and strength after 1967 through their terrorist acts in Israel and through the appeal of their doctrines. The Palestinian guerrillas soon found themselves in collision with the governments of Lebanon and Jordan, which opposed them because their activities stimulated Israeli retaliations and, in the case of Jordan, because the Palestinians tended toward internal revolution. The result was the bitter war between the Palestinians and the Jordanian monarchy in 1970-71. Following King Hussein's victory, the guerrillas entered a period of decline. The net result was the failure of the Palestinians "to carry out revolutionary guerilla warfare on the Algerian and Viet Cong patterns inside the Israeli occupied territories" (p. 228).

The book is based on Western public sources (and documentation is sparse), but this limitation is one shared by all who seek to study the military side of this topic. More serious is O'Ballance's unfamiliarity with Arab history and politics. The occasional shots at earlier periods miss the mark, and the account of the political side of the Palestinian movement suffers from a few errors and some confusion. The author incorrectly explains "Fatah" and does not give a reliable account of the Palestinian organizations. (He uses two English names for a single organization.) Finally, he fails to make a systematic analysis of the available data relative to the effectiveness of Palestinian guerrilla activities. In these respects his work is inferior to

The Politics of Palestinian Nationalism by William B. Quandt, Fuad Jabber, and Ann Mosely Lesch (1973). But O'Ballance's book meets the need for a detailed, coherent narrative of Palestinian military activities, a narrative constructed by fair-minded and skillful use of the available sources.

C. ERNEST DAWN
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EDGAR O'BALLANCE. *The Electronic War in the Middle East, 1968-70*. [Hamden, Conn.:] Archon Books. 1974. Pp. 148. \$8.00.

Edgar O'Ballance discusses what he contends was history's first all-out electronic war and in the process assists the reader in developing a more accurate perspective regarding the events which led up to that conflict. Of particular significance is O'Ballance's outline of the role of the superpowers vis-à-vis their client states—those that conducted the hostilities as the United States and Soviet Union supplied necessary equipment and key manpower.

In terms of military affairs, O'Ballance rightly points out that the "electronic summer" of 1970 changed the pattern of future warfare in the Arab-Israeli conflict. The role of sophisticated air defenses, electronic countermeasures (ECM), and even electronic counter-countermeasures (ECCM) was to have a powerful impact on subsequent military preparations by the two warring sides. The author highlights the fact that the advent of such weaponry increased the need for more specialized technical training. Thus, while the number of available personnel may have become less important, the quality of manpower has become a paramount concern.

One particularly important message is implicit throughout the book—the folly of assuming that if one side becomes militarily superior, this imbalance of power somehow will prevent warfare from erupting. Such a situation usually results in a change of tactics, as in the case of Cairo's reaction to the destruction of its air force by Tel Aviv in the massive Israeli pre-emptive strike in June 1967. Rendered defenseless against deep-penetration raids, the Egyptians turned to the Soviet Union for an effective air defense system. The United States then attempted to assist its client state (Israel) to maintain superiority by providing it with highly sophisticated ECM to counter Egypt's Russian-built and Russian-manned defenses. And the cycle continued.

The author aptly concludes his study by stating that "electronics in warfare have come to stay and their degree of involvement will only be limited by the wealth, resources and technical capability of

the country concerned, or its ability, like that of Egypt and Israel, to persuade larger nations to supply them with military means."

Although O'Ballance wrote the book prior to the outbreak of hostilities in October 1973, his assessments for the future were not proven inaccurate by the war, and his book should be considered required reading for those interested in military history, the Arab-Israeli conflict, or arms races.

DALE R. TAHTINEN

American Enterprise Institute

AFRICA

CHARLES WENDELL. *The Evolution of the Egyptian National Image: From Its Origins to Ahmad Luṭfī al-Sayyid*. (Published under the auspices of the Near Eastern Center, University of California, Los Angeles.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 329. \$17.50.

This study traces the historical evolution of the nationalist idea in Arab political life from its inception in the Umma Muhammadiyya, or the international community of believers, through its Islamic reformist phase. It ends with the ideas of secular nationalism as expounded in the journalistic writings of the liberal Egyptian thinker, Ahmad Luṭfī al-Sayyid (1872–1963).

Charles Wendell's erudite exposition and lively narrative convincingly demonstrate the intellectual inadequacies of traditionally based and religiously inspired political belief systems that gradually emerged in the eighteenth and nineteenth centuries as indigenous responses to the crushing dominance of European secularism. Wendell analyzes the historical process of challenge and change in the specific context of the Egyptian experience, viewing it simply as a microcosm of an intellectual phenomenon affecting the whole of the Arab East. From the period of Napoleon's invasion of Egypt in 1798 until the eve of World War I the focus of Arab nationalist aspirations successively narrows from the universalist Islamic state to the European-imposed, self-contained, and autonomous nation-state.

The clearest and most eloquent definition and defense of a liberal, rationalist view of the Egyptian *umma* or nation-state is nowhere better articulated than in the writings of Luṭfī al-Sayyid. Although neither an original thinker nor a philosopher, among a generation of educated Egyptians he was known as "ṣailasuf al-jil," philosopher of the generation. His contribution remains in the manner in which he expressed European ideas and sought their incorporation into a uniquely defined Egyptian nationalist image that rejected purely pan-Ottoman, pan-Arab, and pan-

Islamic political formulations. Indeed, what distinguished Luṭfī from his intellectual predecessors was his advocacy of European legal and political concepts of citizenship contained in a territorially defined nation-state.

For Wendell, Luṭfī's liberalism, while genuine, was misplaced and overly idealistic. Moreover, it borrowed too heavily from Europe ever to achieve lasting acceptance among a tradition-bound and profoundly Islamic mass public. European-inspired ideas of liberal, secular nationalism were thus doomed to inevitable extinction in an area where religious zeal and political extremism historically prevailed. Clearly the postrevolutionary period of Nasserite rule would give support to such a view. However, Sadat's Egypt appears to be invoking the spirit, if not the letter, of many of the liberal nationalist ideas that Luṭfī so energetically put forth in the pages of *Al-Jarida*, which he edited from 1907 to 1914. If in fact this trend continues, it may confirm and give contemporary relevance to Luṭfī's essential belief in the distinctive Egyptian view of its political self—a free, liberal spirit, culturally aligned to the West, but respecting its twin inheritances from the Arab and Pharaonic pasts.

In conjunction with the earlier pathbreaking studies of Jamal Mohammed Ahmed, Nadav Saffran, and Albert Hourani, Wendell's meticulously researched and balanced presentation provides another splendid addition to the growing literature in English on the intellectual genesis of Arab and Egyptian political and social thought in the modern period.

JOHN P. ENTELIS

Fordham University

ALEXANDER SCHÖLCH. *Ägypten den Ägyptern! Die politische und gesellschaftliche Krise der Jahre 1878–1882 in Ägypten*. Foreword by ALBERT HOURANI. (Beiträge zur Kolonial- und Überseegeschichte, number 9.) [Zurich:] Atlantis. 1972. Pp. 396. 48 S. fr.

"Egypt to the Egyptians!" This, the Monroe Doctrine in Egypt, was the slogan of a movement there that preceded and led to, but was then strengthened by, the British invasion of that country in July 1882. Yet, the 'Urābī movement, named for its most influential promoter and spokesman, had neither narrowly defined chauvinistic nor racist goals. It did not intend to question the authority of the sultan in Istanbul, who was still the nominal Egyptian sovereign, nor to establish an Egyptian national state, nor even to overthrow the Egyptian khedive, a fact the khedive himself did not realize. Rather, the movement's first objective was the abolition of European financial and political control over Egypt, which was the result of Egypt's £91 million indebtedness in 1876. It also aimed at

the abolition of privileges, especially in the army, of the old Turko-Circassian ruling class, which had regained much terrain since Muhammad 'Alī's blow against it in 1811. When the English army occupied Egypt, it did so for strategic and financial interests and also for the khedive, who was reinstated. That there were influential Egyptian personalities on the invaders' side shows that the 'Urābī movement, in its fight against the foreign aggressors, was not able to unite the leading groups of Egypt.

Alexander Schölch describes and analyzes the Egyptian political and social crisis of 1878-82 as well as the development of the 'Urābī movement. Schölch spent years working in European and Near Eastern libraries and archives before presenting this study for his doctoral dissertation at Heidelberg University in 1972. He introduces his work with an analysis of Egyptian society, emphasizing the leading regional and national group under Ismā'īl (1863-79). He then describes the overthrow of the social order by means of foreign intervention, the new politically influential groups after the overthrow, and, finally, the termination of change in Egypt caused by the English occupation.

Schölch's exciting and readable book offers a magnificent case study of nineteenth-century European imperialism: growing indebtedness, for whatever reasons, "necessitated" foreign financial control. It was the supervisors' evaluation of the political and social changes that finally caused military intervention. The people who profited most in the end from this development were the European merchants and bankers.

HARTMUT FÄHNDRICH
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TADEUSZ LEWICKI, with the assistance of MARION JOHNSON. *West African Food in the Middle Ages, According to Arabic Sources*. New York: Cambridge University Press, 1974. Pp. xv, 262. \$23.50.

This, the English version of a study that appeared in Polish in 1963, is a welcome addition to the literature. What we know of the precolonial economic history of the forty-odd tropical African countries has heretofore been limited to the last four and a half centuries. With evidence from little-used Arabic sources, Lewicki pushes back what we know about West African economic history another five centuries.

Although the scholarship is generally solid, the book changes very little our understanding of West African economic history. For the most part it merely confirms earlier speculations, and, even so, it is limited in geographical coverage largely to the Sahara Desert and its southern fringes. We still

know little about economic change in the forest and savanna belts of West Africa—now by far the most important areas in terms of economic activities—prior to the nineteenth century.

The main contribution of the book, then, is that it confirms what we would have expected from previous evidence. We now have confirmation that the millets and sorghums, which today account for over eighty percent of all calories consumed in the desert fringe portion of the area studied, were also the dominant staples at least a thousand years ago, and that persons living in the area were familiar with a majority of the less-important crops now found there for at least five hundred years.

Unfortunately, the study tells us little about other aspects of the economy. There is one reference to use of irrigation for producing wheat in the eleventh century (p. 39), but otherwise almost no information on techniques of production, the organization of labor, marketing, or transport systems. Nevertheless, it is good to have the Arabic sources finally searched and the findings made available in one of the world's major languages.

MARVIN MIRACLE
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Madison

JOHN D. HARGREAVES. *West Africa Partitioned*. Volume 1, *The Loaded Pause, 1885-1889*. Madison: University of Wisconsin Press, 1975. Pp. xiv, 273. \$17.50.

In what is explicitly the first of three volumes, John D. Hargreaves gives us a detailed historical narrative covering five crucial years of French, British, and German involvement in West Africa. He addresses the "dramatically sudden transition" to a European imperial role in the area from Senegal to Cameroun. Hargreaves provides some answers to the question of why this transition took place "despite the absence of sustained or broad-based pressures for the establishment of empire in Africa."

Readers of Hargreaves' *Prelude to Partition* (1963) will expect detailed scholarship, narrative control, and generalizations framed with knowledge of wider theoretical controversy. They will not be disappointed with this volume, although Hargreaves leaves the ideological and bibliographical dimensions of imperialism in West Africa for the third volume of the trilogy. Volume 1 opens with a summary of European-African relations in 1885. It proceeds with discussions of French inhibitions and initiatives concerning Samori, the Senegambia, and the Kong; the anomalies of the regime set up in the Oil Rivers region by the Foreign Office; the complexities involving Yoruba, Dahomeans, the Aja, and Lagosians on the Slave Coast; and the Sierra Leone hinterland. It ends with a recapitula-

tion of the 1885–89 period. Hargreaves notes particularly the confusion of purpose among metropolitan groups and the great diversity of interests of African participants.

In concluding, the author suggests that the increasing European involvement of these years was not brought about by metropolitan plan. There was not then an imperial design. In each case “some effective driving force can be identified within the frontiers of existing colonial societies.” Practitioners became spokesmen; merchants, missionaries, local officials, and African leaders sought increased contact and wider opportunities. The “inner logic of frontier relationships” extended European commitments and made governments wary of the others’ “agents.” The desire to forestall future, mostly illusory, economic benefit that would fall to others had some persuasive effect, but in essence it was local pull, not imperial push, that brought Europeans and Africans into inextricable relationships. Even by 1889 Europeans did not think of political dominion as the only appropriate solution for West African problems.

Hargreaves has set the stage. We await with high expectations the curtain’s rise on African and European decisions during the final decade of the nineteenth century.

PROSSER GIFFORD
Amherst College

PHILIP D. CURTIN. *Economic Change in Precolonial Africa*. Volume 1, *Senegambia in the Era of the Slave Trade*; volume 2, *Supplementary Evidence*. Madison: University of Wisconsin Press. 1975. Pp. xxix, 363; xi, 150. \$15.00, \$15.00.

Philip D. Curtin refers to this two-volume study as “an experiment in a kind of history sometimes called interdisciplinary” (p. xix). The result of the experiment is a seminal work of historical, economic anthropology. Using theories and concepts borrowed from anthropology, economics, geography, and sociology, Curtin reconstructs the process of economic historical change in a West African region known as Senegambia from approximately the middle of the sixteenth century to about 1850. If the book is an experiment in historical methodology, it also is a study of how a fringe economy of the Western world functioned before it fell under colonial domination. Curtin’s conclusions challenge many sacred beliefs concerning the function of “primitive” economies. His model of articulating and changing economies should replace Karl Polanyi’s earlier substantivist model published in *Dahomey and the Slave Trade: An Analysis of an Archaic Economy* (1966).

Curtin reconstructs his data on two levels, which provide new perspectives. On one level he views

Senegambian historical economic phenomena from a universal perspective, relating events to “the process of change in human society generally”; on the other he reconstructs history from a precolonial Senegambian orientation. Accordingly, Curtin eschews the Scylla of “a perspective . . . dictated from America,” including its ethnocentrism, and the Charybdis of focusing on great men, great ideas, and great events. Such treatment makes this not a study of “savage Africa,” “static Africa,” “primitive Africa,” or a “waiting-to-be-civilized-Africa,” but an imaginative recovery of the broader patterns of precolonial Senegambian life, which emphasizes economic and commercial pursuits, including the Atlantic slave trade.

Curtin divides the subject into eight chapters forming a continuous whole. The first chapter delineates regional political perspectives, the next three deal with Senegambian and European trade. Here the treatment is refreshing, for instead of the traditional historian’s one-dimensional study of “European trade on the West African coast,” two-dimensional, interdependent, Afro-European trading diasporas emerge with interlocking points at coastal and interior commercial enclaves. Curtin justifies his Senegambian perspective by arguing that without West African trade diasporas of itinerant and sedentary merchants, who established and maintained interior trade routes and networks, the commercial scope of corresponding European trading diasporas, including that of the Atlantic slave trade, would have been severely limited.

The African-oriented fifth, sixth, and seventh chapters provide accounts of historical change in precolonial Senegambian economies. Curtin discusses raw materials and commodity production for internal and external market exchanges as well as currencies, prices, price fluctuations, and the introduction of new currencies in the mid-eighteenth century. Here he challenges the myth that precolonial African resource exchange was effected through barter. After discussing existing concepts of “primitive” and “modern” money, he concludes that “anything can be money as long as the people who use it accept it as a recognized medium of exchange and standard of value” (p. 235). Finally, he examines the process of competition—yes, competition—in indigenous West African trade.

Curtin loves his work, and it shows. Some, like Lamin Sanneh (“The Origins of Clericalism in West African Islam,” *Journal of African History*, 17 [1976]: 49–72), criticize—perhaps with justification—Curtin’s “liberal” interpretations of data. However, when faced with a seminal study that generates a new model for inquiries into precolonial African economics while justifying an interdisciplinary methodology, one must cast aside

chiding and scolding and express appreciation to the author.

B. MARIE PERINBAM
University of Maryland,
College Park

DAVID ROBINSON. *Chiefs and Clerics: Abdul Bokar Kan and Futa Toro, 1853-1891*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1975. Pp. xi, 239. \$19.50.

In the 1860s as Futa Toro—a section of the middle valley of the Senegal River 240 miles long and 10 miles wide—disintegrated into political chaos, a young war leader named Abdul Bokar Kan began a bid for political power at a provincial level. For the next thirty years his career was bounded by the three major realities of Futa political life: the ambitions of local Futa leaders and their neighbors, the manifestations of Islamic reform, and the expansion of French interests in the Senegambia.

David Robinson's purpose in this book is to challenge the assumption that Futa Toro was a "passive receptacle of external initiatives" caught between two expansive empires. In seeking to show how Abdul Bokar Kan established a measure of autonomy, the author demonstrates a range of complex interactions between Europeans and Africans during the late nineteenth century.

Chiefs and Clerics is strongest in its chronology of the politics of Senegal's narrow central valley during these forty years. It is a meticulous account of who joined whom and who fought where as the slow growth of the colonial bacillus progressively destroyed the options open to Futa's inhabitants. It is a thoroughly documented account, and the footnotes often provide reading as vivid as the well-written, crisply directed text.

In Abdul Bokar Kan, David Robinson has picked a protagonist largely ignored in earlier histories of the Senegambia. A provincial warrior who was never able to solve the essential problems of the region or to create new institutions, Kan survived by playing a lone hand in the turbulent precolonial period. He is a difficult subject to portray from the often enigmatic oral traditions and colonial reports. Had he been a stronger personality, Robinson's task would have been easier. He might have been able to draw the book more fully out of the realm of local history had he developed an analytic theme. One could wish, as well, for a discussion of the social climate and institutions out of which this historical chronology emerged and within which Abdul Bokar Kan acted out his drama.

CHARLOTTE A. QUINN
West Redding, Connecticut

JOHN GRACE. *Domestic Slavery in West Africa: With Particular Reference to the Sierra Leone Protectorate,*

1896-1927. New York: Barnes and Noble. 1975. Pp. 294. \$23.50.

Domestic slavery was not abolished in Sierra Leone until 1926. It is ironic that the colony founded as a base in the crusade against the slave trade was the last place in the British empire where slavery was legal. John Grace's book is a study of the reasons for this bizarre epilogue to Britain's abolitionist crusade.

The intellectual basis for British reluctance to abolish slavery in Sierra Leone was what Grace calls "conservationism"—the argument that it was either immoral or unwise for a colonial power to tamper with the customs of the colonized. Conservationism was generally a justification for avoiding socially disruptive policies that would raise the cost of administration. Grace argues that two problems lay behind the failure to abolish: the need to shore up the power of the chiefs, who were major slaveholders, and the fear that abolition would lead to a decline in agricultural production. When abolition finally came, it was forced from London because Britain feared embarrassment in the League of Nations.

Though generally well researched and well argued, Grace's book has several shortcomings. The most important is that the author does not seem to have done any significant interviewing of former slaves or slaveholders in Sierra Leone. This results in a failure to get behind the façade of law and official rhetoric into concrete social and economic structures—in particular, the slave's position in the household. Grace does not analyze in any detail the slave's labor obligations, his rights to land, or his rights to his offspring. Having failed to study household relations, he does not recognize changes in social relationships that undoubtedly occurred.

Grace echoes the "conservationists" in telling us that African domestic slavery was not a harsh institution, but this oft-stated remark seems to be in contradiction to one of the major themes of his book, the constant problems caused by runaway slaves. Sierra Leonean slavery may not have been as harsh as slavery in the fields of Jamaica or the Attic silver mines, but the slaves do not seem to have liked it. Yet, when abolition finally came, most slaves stayed where they were. Why? Had changes already taken place or did abolition force changes that made their social position more acceptable? Or did they have few options? This is social history and these are the questions Grace does not answer.

MARTIN A. KLEIN
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S. O. BIOBAKU, editor. *Sources of Yoruba History*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1973. Pp. vi, 268. \$11.25.

The Yoruba of southwestern Nigeria have been better served by historians than any other ethnic group in tropical Africa. They have been the object of a historical research scheme of which this volume is a by-product.

The reconstruction of the precolonial history of most tropical African societies depends largely, sometimes wholly, on nondocumentary sources, so that a volume which sets out to assess "the various categories of source material available for the reconstruction of Yoruba history" should prove useful to those interested in precolonial African history. Unfortunately, the treatment here of the various sources—traditional history, oral literature, ceremonies, archeology, art, language, sociopolitical structure, warfare and weapons—is variable. Some contributors, perhaps because of lack of editorial directive, have concerned themselves more with a description of the sources than with what these can tell the historian. This is particularly true of the otherwise excellent studies by Robert Smith, "Yoruba Warfare and Weapons," and Dennis Williams, "Art in Metal." This latter consequently compares unfavorably with Carroll's companion piece, "Art in Wood," that addresses itself directly to the historical evidence provided by Yoruba carving.

Law's chapters on contemporary written sources and traditional history are masterly studies of the problems inherent in using both types of sources. He argues cogently that written sources are not necessarily more reliable than oral sources, a point that is conventionally accepted, but rarely as effectively illustrated.

Willett competently describes the service archeology has rendered Yoruba history, while Peter Lloyd examines what historians can learn from comparisons of the widely differing sociopolitical structures found in Yorubaland.

Of the three chapters covering the prolific oral literature of the Yoruba, only Delano's gives a really satisfactory treatment of it as a historical source. Adetugbo's admirable essay on language shows how its study can help steer the historian through the technicalities of linguistics to an understanding of the limitations and potentialities of this source.

Ade Obayemi, a Yoruba historian-cum-archeologist, has recently provided an outstanding demonstration in the volume edited by J. F. Ade Ajayi and Michael Crowder (*History of West Africa* [1976]) of how the various sources covered by this volume can successfully be combined to reconstruct early Yoruba history. It is sad, therefore, that one of the Yoruba historians associated with the Yoruba Historical Research Scheme—the editor himself, or I. A. Akinjogbin—did not conclude the present volume with a chapter on the problems

the historian faces in using sources from disciplines in which he has no formal training.

MICHAEL CROWDER
University of Lagos

PATRICK COLE. *Modern and Traditional Elites in the Politics of Lagos*. (African Studies Series, number 12.) New York: Cambridge University Press. 1975. Pp. xi, 297. \$17.50.

Precolonial elites in Africa did not disappear under the weight of alien rule; they did not become mere pawns of the Europeans, nor were they driven by atavistic impulse when they opposed them. Rather, they continued to calculate and to pursue their economic and political interests, adjusting to the new context of the colonial state. Other elites, created by colonialism and content to work with the imperialist as long as they gained power and wealth from the skills they learned, challenged traditional chiefs and kings at the beginning of British rule. European attempts in the 1890s to seize land controlled by chiefs in Lagos and then to weaken the chiefs' positions as middlemen-traders within the new order coincided with white civil servants' efforts to prevent the enthusiastic African scramble up the colonial ladder and with the churches' growing racism. The result from the last decade of the nineteenth century through World War II was the wedding of the modernizers' skills with the traditionalists' legitimacy into an opposition movement that Patrick Cole labels the first Nigerian nationalism.

By delving deeply into public and private archives, this Nigerian historian has come up with a detailed analysis of the Oluwa land case (1913–26), the first victory of traditional landholders that also brought Herbert Macauley, the educated grandson of an African bishop, to prominence. By examining the Eleko Affair (1920–31), a dispute over the *Oba*-ship (kingship) of Lagos, he shows how the newly mobilized traditional and modern elites formed a political movement and then a party. Thus, Cole brings to light new information about the Ilu Committee and the Nigerian National Democratic party (NNDP) and thereby joins G. Wesley Johnson and others in challenging the idea that politics in Africa began only after World War II.

Although the author proves that politics existed in Lagos, his evidence that it was Nigerian nationalism is incomplete. Two slim references to the NCNC and Action Group that led Nigeria to independence are insufficient to prove the connection between prewar articulation of elite interest and postwar sovereignty. And when he accuses scholars such as Coleman, Apter, and Young of allegedly ignoring the roots of nationalism, is he not

committing a similar offense by not taking into account changes going on outside of Lagos among non-Yoruba peoples?

The book bears strong traces of the dissertation—86 pages of rich footnotes compared with 191 pages of text—but polemics intrude. To dismiss Henry Carr, who opposed Macauley, as “Malcolm X’s House Negro” without mentioning his contribution to the modernization of Nigerian education is much more than an editorial blunder. Lacking balance and attention to nuance, the book is nonetheless a useful study of African elite behavior.

BRIAN WEINSTEIN
*Woodrow Wilson International
Center for Scholars*

T. O. RANGER and JOHN WELLER, editors. *Themes in the Christian History of Central Africa*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xvi, 285. \$16.50.

This book presents deep but disjointed studies of a variety of themes grouped in three parts. Part 1 deals with the interaction between Christianity and Central African religions (though the latter are never defined). Consequently we get descriptive sketches of cultic movements, notably the M’Bona Cult, the Nyau Societies, the Mwana Lesa movement, and “heathen practices.” This superficial concentration on cults gives an unsatisfactory notion of religious interaction, since Christianity has interacted with African religions on many more and deeper levels. Part 2 describes Christianity and colonial society, focusing on the achievements of the Livingstonia Mission, the Epworth Mission, and three elitist Church leaders (A. S. Cripps, J. L. Membe, and A. May). Part 3 presents Christianity and contemporary society in Zimbabwe (Rhodesia). The book closes with two very useful indexes, general and thematic.

Themes in the Christian History of Central Africa has many of the normal scholarly qualities, and it is clear, readable, and informative. However, it presents only case histories, as if history were just a mosaic of special, bounded events. It assumes that other historical methods should “seek a more adequate balance,” and it brings attention to “the need for ‘inward-looking’ history.”

Of the fifteen essays and introductions, only two are written by Africans, and yet the book claims to present a “Christian history of Central Africa.” Thus, it is largely an outsider’s history and therefore an affront to African peoples and scholars. Can the foreigner, who also has been a colonizer, really describe and interpret meaningfully the history of those he colonized? One need only read the two essays by African contributors to see that

they describe a different historical environment—a living environment in which one hears the voices of the people and not the passing whistling of benevolent foreigners. Even if this book is academically sound, it is not “ideologically” convincing. At best, then, it is only an appetizer; it cannot fully satisfy the reader, despite the claims the editors make about it. And because it cannot, one must put an inevitable question mark upon its credibility and usefulness.

JOHN MBITI
*Ecumenical Institute,
Geneva*

EDOUARD BUSTIN. *Lunda under Belgian Rule: The Politics of Ethnicity*. Cambridge, Mass.: Harvard University Press. 1975. Pp. xiv, 303. \$11.00.

With this important and probably definitive volume, Edouard Bustin proves that a scholar in political science need not be a prisoner of jargon or of theory and can have much to contribute to historical scholarship. Bustin produces a careful, two-sided study; it is both a modern history (mainly 1900–65) of the Lunda kingship, an important traditional African kingdom in southwest Zaïre (formerly the Belgian Congo), and a precise case study of the nature and impact of colonial rule on a largely rural section of Central Africa. Both topics are naturally connected, and the author succeeds in illuminating them while keeping in perspective African history and politics.

The first two chapters discuss the Lunda state’s murky origins and its history up to 1891 when the Belgians began to consolidate their hold. The heart of the fully documented study is in the next five chapters, which deal with the evolution of the weakened Lunda state and its relationship with the Belgian administration up to 1960, and with the independence of the country as the Republic of Congo (Leopoldville). The next-to-last chapter briefly discusses the first five years of independence, when the Lunda state became involved in a civil war, sparked by the secession of the Katanga province under the extraordinary local politician, Moïse Tshombe.

Bustin’s base of historical documentation is impressively thorough and varied: personal interviewing in the field, anthropological and historical scholarship, relevant provincial archives in Zaïre, and colonial archives in Belgium. The marrying of archival and field work is most felicitous.

This study is particularly valuable for its focus upon a neglected topic and period: rural conditions during the interwar period (1919–39). It will also interest those concerned with its discussion of ethnic feeling and African nationalism outside of the better-studied urban areas of Zaïre. The most

interesting and original chapters were chapter 6 ("Out of the Depression and through the War; The Bureaucratizing of Chieftaincy"); chapter 7 ("From Tribalism to Ethnicity: The Growth of Lunda and Cokwe National Sentiments"), and chapter 8 ("Decolonization, Secession, Reunification: The Lunda and Cokwe through the Congo Crisis"). African historians will find here some familiar themes, especially the ethnic conflicts and the artificiality of the old colonial frontiers, which were adopted wholesale by the new African states.

DOUGLAS L. WHEELER
University of New Hampshire

ANDREW D. ROBERTS. *A History of the Bemba: Political Growth and Change in North-eastern Zambia before 1900*. Madison: University of Wisconsin Press. 1973. Pp. xxxiv, 420. \$15.00.

Concentrating on the period prior to 1900, Andrew Roberts contributes a useful and often probing history of the Bemba peoples. He bases his study on extensive field work in Zambia during 1964 and 1965, skillfully blending oral traditional and linguistic, anthropological, and historical sources.

Roberts is best at describing the flow of Bemba history. Anyone who has ever struggled with the problems of African prehistory will appreciate his painstaking reconstruction of the chronology of the area. However, his analysis of the Bemba political system and its evolution, while quite properly redressing the previous misperceptions concerning the centralized character of its organization, is somewhat sketchy and in places unmindful of existing works dealing generally with traditional political systems. One comes away with a less than clear-cut understanding of the internal dynamics of the system, even though there is considerable detail concerning its operation.

Numerous charts, maps, and illustrations enhance the work; the footnote form (at the bottom of each page) will delight the interested reader. The book will be of interest primarily to those concerned with the history of Zambia and the formation of African political units.

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GODFREY MURIUKI. *A History of the Kikuyu, 1500-1900*. New York: Oxford University Press. 1974. Pp. viii, 190. \$10.50.

Within the last decade an increasing number of historians have produced studies of uncentralized East African societies. Of these, Godfrey Muriuki's treatment of the Kikuyu must be considered among the best. A Kikuyu himself, Muriuki presents deep insight into this somewhat exemplary

Bantu stateless society. In doing so he has produced something more valuable than East African scholars eager to know more about the Kikuyu would expect. His book is a model of historical analysis of structural and institutional development, especially pertinent to descriptions of the evolution and functions of social structure.

Many readers will be particularly interested in the author's efforts to dispel a number of tenacious misconceptions about Kikuyu relations with their neighbors. For instance, the Maasai and Kikuyu were not traditional enemies. Where they had mutual contact, they lived in a state of symbiosis. Trouble between them developed only when economic hardship forced the Maasai onto Kikuyu land and when the Imperial British East Africa Company began to take sides. The author also shows that the Europeans did not initiate the land alienation which caused the Kikuyu such grief; they did, however, intensely exacerbate it. This observation is a good example of the author's objective consciousness.

The detail and intricacy of the description of migrations and settlement of the many subgroups of the Kikuyu composite is quite impressive. The thoughtfulness and depth of comprehension of the impact of the age-set system (*riika*) on Kikuyu society is where the value of this study as a model becomes most obvious.

Little detracts from the high quality of this book, but, in the spirit of balance, it should be pointed out that the author has failed to emphasize the important distinction between the pastoral Maasai and Iloikop, a point that has always been crucial in Maasai-Bantu relations. Also, I have found Muriuki's explanation of the origin of circumcision practice and the age-set system rather confusing and lacking in solid evidence.

It should be obvious, however, that this is a very useful book. I look forward to more high-quality scholarship from the author.

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EDWARD A. ALPERS. *Ivory and Slaves: Changing Patterns of International Trade in East Central Africa to the Later Nineteenth Century*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xviii, 296. \$13.50.

As defined here, East Central Africa stretches from the coast of northern Mozambique and southern Tanzania inland to, and somewhat beyond, Lake Nyasa. While Alpers offers some revisions to accepted interpretations of the Portuguese impact on the East African coast in the sixteenth and seventeenth centuries, he concentrates on the eighteenth

century. A fascinating account emerges of the changing success of the Portuguese at Mozambique Island—relative to Kilwa, four hundred miles north on the Swahili coast, which Portugal lost in 1698—in attracting the overland ivory caravans, especially those of the Yao. Kilwa eventually became the preferred terminus, not because it was closer than Mozambique to the main sources of ivory, but because traders there offered better prices. Even at Mozambique, the Portuguese colonists found it difficult to compete with Indian merchants who had made themselves indispensable to the conduct of trade in general. With the effective opening of the East African slave trade in the late eighteenth century, at first to meet local demands in the Indian Ocean but during the “abolition” period to supplement supplies to the New World as well, Mozambique found a new line of business. Here again, though, Kilwa and the Swahili coast were not outdone. By tracing the slave trade into the nineteenth century, Alpers is able to bring to bear a more complex and exciting historiography.

But Alpers’ most remarkable achievement is his original research on the eighteenth century in a hitherto unknown region. Whereas historians have studied the coast—though less well for the eighteenth than for preceding centuries—and the lower Zambezi Valley, they have totally neglected the hinterlands of Mozambique and Kilwa. The extreme rarity of firsthand, written sources before the mid-nineteenth century, the “stateless” nature of most societies of this region, and the perverse configuration of colonial boundaries have all conspired to discourage historical treatment and fieldwork. Though Alpers has not done the fieldwork and oral research, he provides the context for others equipped to embark upon it. His own primary work has been in archival sources, notably those in Lisbon, relating to Mozambique Island and the coast in general. What these suggest about the interior he correlates with more recently published ethnographic materials.

The book is based on Alpers’ 1966 London University doctoral thesis. Since then he has rejected some of the then-fashionable assumptions on local initiative and development in response to international trade, and he has reframed the work in terms of underdevelopment. This is interesting, though he occasionally stresses the obvious. As he admits, he is limited by the available evidence. But once the theme of underdevelopment loses its luster to more advanced theories, one hopes that the basic materials Alpers presents here will prove sufficient for further testing.

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J. FORBES MUNRO. *Colonial Rule and the Kamba: Social Change in the Kenya Highlands, 1889–1939*. (Oxford Studies in African Affairs.) London: Oxford University Press. 1975. Pp. vi, 276. \$19.50.

ROBERT L. TIGNOR. *The Colonial Transformation of Kenya: The Kamba, Kikuyu, and Maasai from 1900 to 1939*. Princeton: Princeton University Press. 1976. Pp. 372. \$25.00.

The area of the Central Highlands of Kenya is an important one in the study of the impact of British colonialism on Africa. Unlike many parts of West and Central Africa or the interlacustrine area of East Africa, the colonial system in the central highlands confronted three politically decentralized societies—the Kamba, Kikuyu, and pastoral Maasai—who had experienced few Europeanizing influences prior to the twentieth century. Moreover, a relatively small but vigorous settler population during the colonial era produced a scheme of dualistic economic development unlike the situation in many other parts of Africa. Until the publication of these two new books, however, little scholarly attention had been paid to the colonial transformation of this area. Robert L. Tignor and J. Forbes Munro have rectified the situation, providing us with a wealth of information and some valuable new interpretations concerning the colonial impact on the peoples of the central highlands and their response to it.

Of the two books, Tignor’s is the more ambitious. Through careful analysis of many interacting political, economic, and social factors present in each society, he is largely successful in attempting a comparative study of the adjustments and reaction of the three societies to the colonial presence. While his approach is undeniably comparative, there is a certain primary focus on the Kikuyu in many areas of the book. This is understandable, however, as he identifies the Kikuyu as the most receptive to, and therefore the most intimately involved with, the various processes of colonial transformation under investigation.

Munro’s focus is more microcosmic. In fact, his title is rather misleading, for it is not the Kamba as a whole upon whom his investigations center, but rather only those of the Machakos district. His basic approach is an interesting one. Avoiding the more typical focus on a transition from a traditional to a modern society during the colonial era, Munro instead employs a “mobilization concept” in developing his central themes. He stresses that this approach not only avoids important terminological problems, but also “illuminates aspects of the colonial situation which can be too easily ignored in the search for tradition and modernity” (p. 248). While Tignor’s examination is well conceived and developed, Munro provides an even

more thoughtful and, in some respects, more analytical examination of the colonial system and the nature of the African response to it.

Each writer bases his interpretations on an impressive array of data, drawn mainly from Kenya and from other archival sources, but also relying to some degree on oral materials. Because many of the same source materials were employed, and because each book examines the same theme in the same area and over the same time period, it is curious that more duplication of information does not occur. In fact, this happens only occasionally, for example, in their respective discussions on the female circumcision controversy (Munro, chapter 8; Tignor, chapter 15) and of the Kamba reaction to the forced destocking of the 1930s (Munro, chapter 11; Tignor, chapter 15). Munro's more specific focus usually insures that his treatment of a given Kamba situation is more detailed than Tignor's, although in a few instances Tignor somewhat surprisingly produces the more thorough examination, as with his discussion of the AIM and its activities among the Kamba (chapter 6 in each book).

Similarly, there are few substantial discrepancies in interpretation. The more significant include the respective investigations of Kamba resistance to wage-earning—Tignor emphasizes the lack of strong collaborative chiefs and an initial absence of severe land hunger (p. 108), while Munro emphasizes the ability to meet cash requirements through their own domestic economy (p. 83 ff.)—and Munro's assertion that Kamba chiefs frequently were succeeded by relatives (p. 70), which Tignor contradicts (p. 60).

While each book has individual strengths, they share a common weakness. Tignor's treatment of colonial chiefs (chapter 3), the labor situation (chapters 5 and 7), and education, with its very impressively researched background information on missionary groups (chapters 6 and 9), are all particularly successful. Likewise, Munro's studies of Kamba chiefs and councils (chapter 4), the build-up of land pressures (chapter 10), and his careful analysis of the process of colonial mobilization of Kamba resources (especially chapter 5) are exceptionally effective. However, in contrast to their careful treatment of the colonial period, the two authors are less successful in their handling of precolonial situations. Tignor, for example, does not appear to have a clear understanding, in some respects, of time-based class systems. One is also dismayed by his implied suggestion (p. 58) that evolution from a decentralized gerontocracy to a government by hereditary chiefs is in some way a natural political process. Similarly, Munro naively presupposes a single origin for the Kamba (p. 8), thereby ignoring a host of recent studies showing

that East African societies have evolved from a complex process of interaction and assimilation between different peoples over considerable periods of time.

Nevertheless, this shared weakness does not seriously detract from the very positive contribution these two books make to the historiography of East Africa. Indeed, the books often tend to complement each other, with Tignor's Kikuyu emphasis and Munro's Machakos Kamba focus providing valuable new insight into those two societies and establishing a sound basis for a very detailed comparative study of their respective reactions to British colonialism. What is required now is that someone produce an equally detailed and thorough examination of the response of the pastoral Maasai.

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ZEWDE GABRE-SELLASSIE. *Yohannes IV of Ethiopia: A Political Biography*. (Oxford Studies in African Affairs.) London: Oxford University Press. 1975. Pp. xiii, 334. \$24.74.

Anyone reading Zewde Gabre-Sellassie's fine political biography of his great-great grandfather, Emperor Yohannes IV (1872-89) is likely to conclude that the monarch was Ethiopia's greatest nineteenth-century politician and the architect of the modern Ethiopian empire. My only reservation about these evaluations is that I claimed them for Menilek II (1889-1913) in *The Life and Times of Menilek II: Ethiopia, 1844-1913* (1975), published a few months before the author's book. Uncannily, we both have used many of the same sources and basically agree with each other on most significant events. Our different analyses must derive from the contrasting locales of our subjects; not only are our focal points different, but we also might disagree about what Ethiopian empire each man unified.

Gabre-Sellassie strongly argues that it took a man like his ancestor—who had an ascetic and even disposition, a profound understanding of internal Ethiopian politics, and the flexibility of personality that would allow him to work with ambitious and fractious subrulers—to solidify the Solomonic crown's authority over traditional "Abyssinia." I concur heartily in this assessment, since I know, better than most, what a difficult man Menilek could be. If Yohannes' major achievement was to resurrect and consolidate patterns of authority in the north, then it was Menilek's deed to expand the state twofold, to its present size, and to incorporate large numbers of non-Semitic speakers, non-Christians, and nonagriculturalists. If Zewde wishes to see the beginnings of

this expansion as part of Yohannes' policy of strengthening his vassals wherever possible, as long as they retained their allegiance to him, so be it. The point is not worth arguing about, even if Menilek undertook his policy of territorial aggrandizement in part to obtain the finances necessary to purchase weapons so that he might topple Yohannes from the imperial throne, and even if most of the expansion occurred after Yohannes' death in 1889. Thus the author and I are both right: Yohannes unified the traditional Solomonic state, and Menilek unified the present Ethiopian empire.

Zewde's book is a vital contribution to the historiography of Ethiopia. Since 1960 Ethiopianists have written a series of good books and articles based upon materials drawn from European sources and Ethiopian documents, both oral and written. The author's research has been thorough, and his bibliography shows the richness of available materials. His use of the hitherto unavailable and little-known Heruy Amharic-language manuscript, "The History of Ethiopia," provides the scholarly world with some interesting insights into and fascinating documentation about the relationship between Yohannes and Menilek, which, by the way, could be used just as easily to derive analyses greatly at variance with the author's constructions. We can be very grateful, therefore, that the author has included several large excerpts in the appendixes. One might inquire, however, why he did not use Nadaw's readily available and valuable Amharic manuscript, "Ras Gobana's History," while he cited others not so important. If space permitted, I probably would have commented upon several small blunders and quibbled over some analytical vagueness that in several cases blurred important issues. I also would have complimented the author concerning his ideas on political assimilation and upon his astute analysis of Menilek's diplomatic relations with Egypt and Italy. Finally, although the author has described his book as a political biography, it is more a study of Ethiopia's unification, revitalization, and national survival in the late nineteenth century, and a good one at that.

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HARRY A. GALEY. *Sir Donald Cameron: Colonial Governor*. Edited by PETER DUIGNAN and LEWIS GANN. (Hoover Colonial Studies; Hoover Institution Publications, number 139.) Stanford: Hoover Institution Press, Stanford University. 1974. Pp. xvii, 181.

The subject of this biography, Governor Donald Cameron, left no diaries. No collection of his private papers exists, and the memoirs that he wrote

were rather reserved and disorganized. Therefore, the materials available for his study are confined essentially to administrative records. With one disappointing exception, the Tanganyika archives, these are the records Harry A. Gailey uses to reconstruct Cameron's life.

Gailey recounts Cameron's services in Tanganyika from 1925 to 1931 and in Nigeria from 1931 to 1935. As governor of Tanganyika, Cameron's one great achievement was to resist plans for "closer union" with the nearby British colonies, especially Kenya, that were aimed at depriving Africans of their land and establishing the kind of settler colony which existed in Kenya. Gailey gives Cameron credit also for his policies toward "native authority." At times the author concedes that these policies were unrealistic, bringing about the imposition of "traditional chiefs" where none had previously existed, but on the whole he judges these policies as having been successful. A reading of J. Gus Liebenow, *Colonial Rule and Political Development in Tanzania: The Case of Makonde* (1971), might have underscored the shortcomings of Cameron's policies. Gailey's account of Cameron's activities in Nigeria confirms previous research on the shortcomings of British policy, especially in regard to their application in the country's northern districts.

While Cameron was an important individual who helped shape the future of the territories he ruled, the lack of records for a critical evaluation hardly makes him an ideal candidate for a biography.

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LEONARD THOMPSON. *Survival in Two Worlds: Moshoe-shoe of Lesotho, 1786-1870*. London: Oxford University Press. 1975. Pp. xx, 389. Cloth \$24.00, paper \$7.00.

In their introduction to the *Oxford History of South Africa* Leonard Thompson and Monica Wilson argue that "the central theme of South African history is interaction between peoples of diverse origins, languages, technologies, ideologies, and social systems, meeting on South African soil." In his present study Thompson emphasizes this theme in recounting the life of an African ruler whose career spanned the Early Iron Age to the Industrial Revolution. As a young man, Moshoe-shoe knew nothing of horses, firearms, or steam power. When he died, diamonds had been discovered in South Africa, railway building had started, and breech-loading rifles were changing the mode of warfare just as complex mining machinery was

altering the art of extracting ores from the ground. The young warrior was familiar only with a society organized in small principalities. The mature statesman had to cope with a situation where African overlords became progressively less capable of maintaining their sovereignty against their white neighbors—Boer and Briton—and where Great Britain seemed bound to dominate South Africa.

Moshoeshoe was the sovereign of a small country, but he was one of the great monarchs of history, comparable to such early medieval state builders as Alfred the Great. Moshoeshoe indeed created the Sotho nation. He defended it against a host of enemies—Boer trekkers, Kora raiders, British fighting men—and he skillfully managed to play his opponents against one another. In maintaining a precarious autonomy for his country the king had to make concessions of a kind found unpalatable by African nationalists of a subsequent generation. He benefited from the new lore brought to his country by Western missionaries. He submitted his country to British protection, thereby opting for the lesser of two evils. Great Britain had no direct interest in exploiting the Basotho, whereas the Orange Free State would gladly have deprived them of their remaining fields and grazing grounds.

In Leonard Thompson the king has found a worthy biographer who provides the first detailed account of the monarch's life in English. The author's work represents a considerable achievement. He skillfully uses information from a broad range of sources: archival, printed, and oral. He also makes good use of anthropological accounts, thereby avoiding the dangers of an approach excessively wedded to one particular discipline, a limitation from which much of South African history has suffered considerably in the past. Above all, Thompson writes in a clear and workmanlike fashion; though he is thoroughly familiar with the anthropological literature, he avoids its jargon. Neither does he indulge in the romanticism that in recent years has affected so much historical writing regarding precolonial Africa. The author gives a sympathetic account of precolonial societies, but he remains aware of their weaknesses. Overpopulation, overstocking, famines, and internal dissensions marked by witchcraft accusations and internecine war were not occasioned by colonial rule, but they were already familiar to the Sotho people of Moshoeshoe's time.

The value of this work has been enhanced by detailed footnotes (notes that appear at the foot of each page), by notes on informants and sources, and by well-chosen illustrations. The author might have omitted his somewhat superficial epilogue, which does not do justice to the study as a whole. Nevertheless, this is the standard work on the sub-

ject. It is not likely to be superseded for a long time.

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MARIAN ROBERTSON. *Diamond Fever: South African Diamond History, 1866-9, from Primary Sources*. New York: Oxford University Press. 1974. Pp. 250. \$22.00.

Scholars dealing with the development of South Africa's diamond industry usually concentrate upon the years after 1870 when the gems became a significant factor in South Africa's economic, social, and intradiplomatic history. Marian Robertson, a feature writer for Radio South Africa, now deals with the first years of the diamond discoveries. She corrects and supplements previous accounts of the Cape government's response and its prior interests in Griqualand West, the site of the major discoveries, which it annexed in 1871. She identifies the government's attitude after the discovery of the first diamond in 1867 as one of "wait and see," an understandable position considering the earlier false hopes of finding profitable mineral deposits, and she notes the development of free-wheeling searching, buying, and selling of diamonds during these years of official nonintervention.

The book, however, is an awkward presentation of her findings. It is much more a stringing together of excerpts from primary sources than a coherent narrative likely to be read by its intended popular audience in South Africa. The text is difficult to follow, even for a fellow researcher wishing to determine the author's internal conclusions. A lack of sufficient maps, a degree of repetition in poorly divided chapters, and a narrowness of approach contrast sharply with the scope and diligence of the author's researches. Her commitment to testing and using primary sources is to be commended, and her lengthy extracts from the papers of Richard Southey (the Cape's colonial secretary at the time) as well as her assemblage of other contemporary sources make sections of the volume and its bibliography worth noting for those working on this period. Since so much needs to be done on both the nature of the mining industry and its relationship to other processes and features of South African history in the last third of the nineteenth century, it is with disappointment that one must give this volume such low marks.

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ANTHONY J. DACHS, editor. *Papers of John Mackenzie*. Johannesburg: Witwatersrand University Press, for African Studies Institute. 1975. Pp. xvi, 282.

Lord Milner, in a characteristic piece of overstatement, once noted that the Reverend John Mackenzie "changed the history of South Africa." This claim will not withstand close scrutiny, yet there is a kernel of truth hidden in Milner's exaggeration. As a missionary who labored for four decades among the peoples of Bechuanaland (modern Botswana) and as a vocal proponent of an enlightened form of British imperial rule, Mackenzie exerted considerable influence in the South African interior during a critical phase of the region's history. This book draws on Mackenzie's extant papers, notably the large collection now housed at the University of the Witwatersrand, in an attempt to delineate the nature of that influence in three distinct areas.

The author sees Mackenzie as a student of African peoples and societies, as a servant of Christianity, and as an imperialist who functioned as a British deputy commissioner in Bechuanaland during the 1880s. This thematic approach, illustrative of three important facets of Mackenzie's career, is a departure from the practice normally followed in volumes of this type. Yet the method employed by Dachs, a capable young Rhodesian historian, merits praise. To be sure, one misses the comprehensive biographical insight that might have been derived from a general cross-section of Mackenzie's papers arranged chronologically, but the editor's choice of material sensibly and sensitively reflects both the missionary and his writings. The coverage is also certain to please all who stress an interdisciplinary approach to the African past. Here is grist for the mills of the social anthropologist, the student of missionary enterprise, those interested in the interaction of strikingly different cultures, and the imperial historian.

The revelation of the wide research potential of the Mackenzie papers is, in fact, the work's single most important feature. Carefully edited and annotated, this volume marks an auspicious beginning for a projected series based on the Witwatersrand University Library's archives. Incidentally, it also builds a persuasive case for a full biography of Mackenzie. That written by Mackenzie's son shortly after his father's death lacks both perspective and impartiality, while Anthony Sillery's recent work suffers from his not having used the more than 3,000 items upon which the present volume is based. Perhaps Dachs will use his background preparation and the abilities displayed in this edited work to produce a complete portrait.

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ASIA AND THE EAST

WM. THEODORE DE BARY *et al.*, editors. *The Unfolding of Neo-Confucianism*. (Studies in Oriental Culture, number 10, Conference on Seventeenth-Century Chinese Thought.) New York: Columbia University Press. 1975. Pp. xiv, 593. Cloth \$20.00, paper \$9.00.

In *The Unfolding of Neo-Confucianism* thirteen scholars have produced a work of immeasurable importance for the entire field of modern Chinese intellectual history. Much more than a sequel to the editor's previous volume, *Self and Society in Ming Thought* (*AHR*, 76 [1971]: 1205), the present book goes beyond the mere introduction of a new perspective to attempt a major revision of the prevailing interpretation of intellectual stagnation in late imperial China. Although he wishes "not to stigmatize [the prevailing view] as a tissue of modern myths and prejudices, superimposed on a misunderstood past," Wm. Theodore de Bary believes that the scholarship represented here will serve to correct our contemporary vision of a China dependent on Western values, forced to remove the dead hand of tradition before it could emerge from an orthodox and culture-bound darkness into the enlightened modern age. Whether or not de Bary's own vision is accurate, the book should have its intended effect. The Neo-Confucian tradition appears as creative and as critical in the seventeenth century as in the eleventh, and the book's collective thesis, that spiritual enlightenment, moral renewal, and the critical methods of "evidential investigation" (*k'ao-cheng*) were mutually supportive and historically linked, is sustained.

Three of the articles, by Wing-tsit Chan, de Bary, and Tang Chun-i, to whom the work is appropriately dedicated, are in fact sequels to their authors' contributions in the earlier volume and provide the interpretation linking the two. Together with Araki Kengo's and Edward Ch'ien's, they argue persuasively that the concept of moral mind was central to the development of Neo-Confucian thought, the seemingly divisive issues relating to ontology, epistemology, practicality, and religiosity notwithstanding. Chan's article, which concludes the volume, points out the emphasis on critical method and systematic doubt in an early eighteenth-century document previously assumed to represent a denial of these basic Neo-Confucian concerns because of its reassertion of state orthodoxy.

If the presentation of a consistent unfolding of ideas based on a faith in personal and social renewal is the strength of this book, the paucity of social or political analysis is its very large weakness. William Atwell's lively article on the political

and literary movement known as *Fu She* concludes that the group was primarily motivated by renaissance ideals. Kristin Yu Greenblatt also suggests that Buddhist moral cultivation appealed to literati interests and led to a florescence of lay Buddhism. Richard Lynn's excellent analysis of poetry as a form of self-cultivation and other such ground-breaking essays all help to humanize our image of the orthodox mind. But none asks why the creative spirit of renewal was never realized in reform, but channeled into orthodoxy, classical research, and other concerns which de Bary calls "totally relevant to the immediate needs of the state, society, and culture that supported them." For the sake of historical truth, as distinct from philosophical or religious, this question must also be answered before the revision is complete. In the meantime scholars can only applaud this substantial contribution to the history of ideas.

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JOHN E. WILLS, JR. *Pepper, Guns, and Parleys: The Dutch East India Company and China, 1622-1681*. (Harvard East Asia Series 75.) Cambridge, Mass.: Harvard University Press. 1974. Pp. xv, 232. \$12.00.

Wills' painstaking but lively study, which is based upon thorough research in the Dutch archives and in published Chinese sources, begins in 1662 with the expulsion of the Dutch from their base on Taiwan by the corsair Zheng Cheng-gong (Koxinga). During the next two decades, while the sea lord's heirs ruled that island, the Dutch sporadically combined with the new Qing dynasty on the Chinese mainland, first to wreak vengeance upon Zheng, and then to gain trading privileges from their own allies. The naval forces of this "uncertain alliance" on several occasions jointly attacked the corsairs, and the Dutch received a biennial trade agreement at Fuzhou; but the "accumulated irritations" of their dealings resulted in the revocation of Dutch privileges in 1666 and the growing conviction on the part of the company's representatives in Batavia that trade with China was a hopeless venture.

During the Three Feudatories' Revolt (1673-81) the Dutch had a second chance. The Qing dynasty was again interested in the company's naval support against Zheng and sent envoys to Batavia in 1679 to offer trading privileges in exchange for military aid. These "farcical" negotiations ended in an impasse. The suspicious Dutch insisted upon guarantees that the envoys were not empowered to give, and the expedition never materialized. By 1681 the Dutch had abandoned their trading post in Fuzhou and thereafter mainly traded with China through intermediaries.

The bulk of Wills' analysis stresses the cultural obstacles to understanding between the Dutch and Chinese, each with their own "diplomatic traditions." Wills tells us, however, that the "irritations" in the 1660s and 1670s were less important causes of the "broken dialogue" than other basic developments that took place at the very end of, or after, the period he has studied. Their victories in Java in 1681 turned the Dutch away from China, while the Kangxi emperor's opening of legal trade in 1684-85 meant Qing abandonment of fickle military alliances for long-term commercial monopolies. There is thus a discrepancy between Wills' assertion that this study of Sino-Dutch relations is about the "emergence of a new political and commercial order in relations between China and the west" in the 1680s and his actual emphasis in the main body of the text upon failures of diplomatic communication before that new order actually emerged. Nevertheless, *Pepper, Guns, and Parleys* corrects the mistaken impression that tributary diplomacy always stemmed from the Chinese sense of cultural superiority, and Wills considerably enriches our knowledge of seventeenth-century Asian history by exposing two decades of the Dutch experience in China.

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ROBERT B. OXNAM. *Ruling from Horseback: Manchu Politics in the Oboi Regency, 1661-1669*. Chicago: University of Chicago Press. 1975. Pp. viii, 250. \$12.50.

The author of this work suggests, with a bow to the late Mary Wright, that the Oboi regency be labeled the "last stand of Manchu conservatism." In the regency period (1661-69), China and the fortunes of the Ch'ing dynasty were at a crossroads. The Sinification policies of early Manchu leaders were subjected to re-evaluation by the regents. And, as the author effectively demonstrates, a nativist reaction flourished as the regents sought "to create an order in which the Manchu system, Manchu officials, and Manchu ideas held undisputed control over Ch'ing China." They succeeded briefly in holding back the tide of Sinification, but when the K'ang-hsi emperor seized power from Oboi in 1669, he built a more harmonious and effective relationship with the Chinese.

Relying largely on official historical and biographical collections, Robert B. Oxnam examines the politics and policies of the Oboi regency against this backdrop of Sinification versus Manchu dominance. The author traces the origin of the regency to the early Manchu wars of conquest and court intrigue, building up an image of the regents

as steeped in Manchu concerns and values. After their appointment in 1661 as regents for the young K'ang-hsi, they began to display their Manchu face to the empire.

In both the metropolitan and provincial bureaucracies, Manchu and Chinese bannermen were elevated over the Chinese official elite, and Manchu-oriented institutions took precedence over their Chinese counterparts. From the Chinese provincial elite, Oxnam argues, the regents demanded absolute obedience and recognition of Manchu superiority. Unable to develop an ideology that could replace Chinese norms of control, they resorted to force in confronting southeastern gentry over tax evasion and Ming loyalism. In their military and foreign policies, however, the regents were strangely passive. According to Oxnam, the regents were faced with military and economic exhaustion after the wars of conquest and felt it was reckless to undertake any new initiatives. They did undertake institutional innovations, however; the most notable example was their governance and defense of Manchuria. Oxnam concludes his work by carefully detailing the shifting balance of power from Oboi to the imperial faction around K'ang-hsi.

Ruling from Horseback is a welcome addition to the growing list of works analyzing the dynamics of alien rule in early Ch'ing China. Oxnam's study provides a more sophisticated view of the Sinification process and points to the important role of K'ang-hsi as the final consolidator of Ch'ing rule.

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PEI HUANG. *Autocracy at Work: A Study of the Yung-cheng Period, 1723-1735*. (East Asian Studies.) Bloomington: Indiana University Press for the International Affairs Center, 1975. Pp. xii, 500. \$15.00.

The reign of the Yung-cheng emperor, spanning the years 1723 to 1735, is of central importance in the study of Chinese autocracy. Building on a number of innovations in the spheres of bureaucratic organization and communications that had been developed by his father (the K'ang-hsi emperor, who ruled from 1661 to 1722), Yung-cheng strengthened imperial power to an extent perhaps unique in his country's history. This power was rational and bureaucratic—far from the tyrannical whims that typified some Chinese rulers before him; though because Yung-cheng was also harsh, and severely mistreated or killed some of his brothers after his accession to the throne, his institutional achievements have been obscured by

dramatic but unsubstantiated charges that he was a usurper.

The first survey of this reign to appear in English was Huang Pei's 1963 Indiana University dissertation, "A Study of the Yung-cheng Period, 1723-1735: The Political Phase." This work drew widely on that remarkable monument to the Yung-cheng emperor's industry, the *Chu-p'i yü-chih* or "vermillion endorsements," a vast collection of memorials with imperial notations—often of considerable length—which appeared in the late 1730s. In view of the importance of this period in Chinese history, it is remarkable that thirteen years later that dissertation—now revised and expanded by a section on "Aspects of Autocracy," and two new chapters on the Banner System and on ethnic minorities in southwest China—is still the only detailed survey of the period. Besides the two new chapters just mentioned, the work has detailed chapters on the succession crisis of 1703-23, the development of the palace memorial system, and the formation of the Grand Council, and it gives as well general coverage to "the literati" and "the populace."

These are all major topics, and worthy of monographic treatment, though there are numerous puzzles in Huang's analysis of them. Why, for instance, if the emperor's efforts to emancipate the "mean people" were ineffective should these measures be called "epoch-making, politically and socially" (p. 20)? Why is he called "the only" Ch'ing emperor to master Ch'an Buddhism when his own grandfather was a passionate believer (p. 33)? How, in the vastly complex politics of the period, can "ordinary officials" stand as a category in analyzing factions (p. 86)? Why should palace memorials be used to weaken censors' powers, when the censors had so little power anyway (p. 119)?

Another disappointing aspect of Huang's book is his failure to consider the most interesting work on the Yung-cheng emperor that has appeared in English since he wrote his dissertation. For in the last few years, besides a good deal of monographic work in Chinese and Japanese on the Yung-cheng reign (which Huang Pei surveys quite thoroughly), two important studies in English have appeared that add new dimensions to our knowledge of the Yung-cheng emperor's character, policies, and abilities. One of these has been published, Silas Hsiu-liang Wu's *Communication and Imperial Control in China: Evolution of the Palace Memorial System, 1693-1735* (1970); the other is Kent Smith's Yale dissertation of 1970, "Ch'ing Policy and the Development of Southwest China: Aspects of Ortai's Governor-Generalship, 1726-1731." Huang Pei's chapter on the Southwest, the major new chapter added since his 1963 dissertation, covers almost exactly the same ground that Smith did, though

Huang does not state if he has drawn on Smith's work, an unfortunate oversight in view of their identical coverage; Huang merely mentions (without comment) one of Smith's briefer papers on the same topic in his bibliography. In the case of Wu's work, Huang is consistently dismissive, both of the monograph on palace memorials and of a number of other interesting and scholarly articles by Wu; the dismissive remarks are kept brief and ambiguous in the footnotes, however, so we do not get any hints as to why Huang keeps repeating older arguments that Wu has been trying for some years to modify. Huang's remarks in his new introductory chapter on "administrative efficiency," for instance, seem to draw significantly on the conclusion to Wu's book, but we are not told whether or not that is so.

Autocracy at Work would have been a better book had it been either less grudging or more analytically rigorous. It is still, however, a work of considerable scholarship, and it will be of help to any Western scholar starting out to explore the Yung-cheng period.

JONATHAN SPENCE
Yale University

JONATHAN PORTER. *Tseng Kuo-fan's Private Bureaucracy*. (China Research Monographs, number 9.) Berkeley: Center for Chinese Studies, University of California. 1972. Pp. 151.

Recent studies of Tseng Kuo-fan, a pivotal figure in modern Chinese history, have turned from the significance of his regional army in China's military history to the importance of his administrative apparatus in the modernization of China's civil government. Tseng created both institutions in the course of suppressing the Taiping Rebellion (1850-64). His administrative structure evolved from what Jonathan Porter calls his "private bureaucracy" and was intended to support the military operations (p. 14). Porter's monograph examines several aspects of this "private bureaucracy" in order to identify its "institutional departures" that, he claims, contributed to China's modernization in the nineteenth century.

Tseng developed his private bureaucracy from the "ambivalent tradition" of the *mu-fu* system (an official's personal staff), which had evolved into an informal, but distinct, institution in the Ming-Ch'ing period. The author, relying on the Weberian criteria for an "ideal bureaucracy," considers Tseng's method of recruiting talent (specialists) as "rational." It was based on "knowledge" and characterized by "impersonality" (p. 40).

The author has helped clarify the inner mechanism of Tseng's private bureaucracy (just as Philip Kuhn has clarified the structure of Tseng's re-

gional army). He finds that Tseng developed his *mu-fu* system into an "inner *mu-fu*," which consisted of Tseng's intimates and associates and was involved with "higher-level" decisions, and an "outer *mu-fu*" of lesser functionaries and attendants. Out of this dualistic system Tseng was able to build his structured and formalized administrative apparatus, characterized by "autonomous bureaucratization."

This is a useful addition to Kenneth Folsom's fairly recent and more comprehensive study on the *mu-fu* system. Nevertheless, the precise link between Tseng's "institutional departures" (the "rational" aspects of his bureaucracy) and the larger problem of China's modernization process in civil government awaits a more serious study in the future. In particular, scholars still need an analysis of the bureaucratic situation and development in the nineteenth century based on a Chinese, rather than a Western, framework.

SILAS WU
Boston College

JOHN K. FAIRBANK, edited and with an introduction by. *The Missionary Enterprise in China and America*. (Harvard Studies in American-East Asian Relations, 6.) Cambridge, Mass.: Harvard University Press. 1974. Pp. 442. \$15.00.

This book consists of twelve articles dealing with various aspects of American missionary work in China. There is an abundance of church archives, missionary writings, and other relevant materials dealing with American missionary work in China, but this is probably the first and the best integrative and interpretive study of such work. John Fairbank mainly discusses three topics: "the American missionary expansion or outward thrust, the impact of this missionary effort on the Chinese people and society, and the backflow of influences affecting the missionaries' home constituency and the American people in general" (p. 6).

The strengths of the work can be enumerated as follows. First, the range of contributors is healthily diverse. While the majority of the contributors are modern Chinese historians, others include historians of American history (James A. Field, Jr. and Arthur M. Schlesinger, jr.), church historians (William R. Hutchison and M. Searle Bates), and even one former influential missionary in China (Bates). The articles by these "outside" authorities are extremely educative and inspiring to students of modern Chinese history. For instance, Hutchison's and Bates' articles explain for almost the first time the mind of the missionaries who went to China. Second, many of the findings in

this work are original and worthy of serious attention. Valentin H. Rabe demonstrates the high quality and noble calling of many missionaries, which should cast doubt on Schlesinger's analytical framework of Prospero versus Caliban in explaining the relationship between missionaries and their converts. Rabe's article suggests that the missionary Prospero did not go abroad to assuage wounded pride at having lost out in his own country. Adrian A. Bennett and Kwang-ching Liu illustrate the debates on Christianity and Confucianism engaged in by the Chinese converts in the nineteenth century. In these debates we see that converts' arguments are by no means all superficial; rather, they are very serious. Therefore, we must reconsider the question as to how many rice-Christians really existed in nineteenth-century China and give more respect to the genuineness of the Chinese faith. Paul A. Varg tries to prove that between 1927 and 1932 American church leaders and missionaries actually shared the same timidity as the American government officials in removing unequal treaties, contradicting our previous belief that church and missionary forces in the 1920s and 1930s were all genuine and enthusiastic supporters of Chinese nationalism.

But some of the findings or conclusions of the work are rather debatable. As for missionary motivations, Field lists the major three: Christ's command, humanitarian compulsion, and romantic adventurism. A perusal of the writings of some major intellectual leaders of the nineteenth century, such as Kidd and Mahan, indicates that one of the major considerations for Western and American outward expansion was to arrest the perils, yellow or otherwise, by Christianizing them. How much influence did this consideration exert upon the missionaries? Recent works by Akira Iriye, Ernest May, and David Healy have shed a different light on this matter.

As for the question of whether missionary endeavor was a failure or success, it seems that many of the contributors of this work would consider it a failure. Was it really? How much success is enough to call it a success? If success means that several hundred missionaries in the nineteenth century and several thousand in the twentieth century should have converted a substantial segment of the Chinese population, then, of course, the endeavor was a failure. But have we asked the question: where else and when in human history has it been that such a meager force (in comparison with the Chinese population) has founded so many schools, hospitals, relief agencies; has transmitted modern thought so effectively to an entirely different culture; and has also converted

one and a half to two million ethnocentrics, which after all is no small number?

Another debatable point of this work is that many contributors have tried to relate the American missionary experience in China to the American fiasco in Vietnam. If we argue that it was a fallacy to use the 1930s experience to justify American intervention in the 1960s, the present attempt might be just another such dangerous historical analogy. Fairbank has this wise rejoinder: "One must . . . be past-minded about the missionaries themselves, for they were Americans of an earlier generation who lived in a far different and simpler world than we face today." He concludes that "it is therefore impossible for today's researchers in this new field to 'do justice' to American Protestant missions in China as a whole" (pp. 15-16). Furthermore, if we agree to this rejoinder, then the accusation that missionaries were cultural imperialists, as Schlesinger has called them, is dangerous and misleading.

The work should also point out many new areas of research for scholars. First, more studies on the Chinese Christians' intellectual responses to Christianity are needed. Philip West's article on Wu Lei-ch'uan of Yenching University gives a fascinating account of how a Chinese Christian leader interpreted Christianity according to Chinese national needs. Any future study should deal with how other Chinese Christian leaders in the twentieth century have thought about and interpreted Christianity. Men like Chao Tzu-ch'en and Hsü Pao-ch'ien, to name a few, deserve serious investigation. Until scholars know their ideas in this matter, they will never know how well or how poorly the Christian message has been transplanted. A topic related to this one could be a study of the Chinese National Christian Council. What role and influence did this organization play in the development of the Chinese Christian movement? Furthermore, Varg's article is a challenge to re-examine our knowledge about the missionaries in the twentieth century. Just how much different was their attitude, spiritual and political, from that of their nineteenth-century comrades? From his article we can see that many missionaries perceived the weaknesses of the Nationalist government and even prophesied the eventual triumph of the Chinese Communists. This should make us, the intellectual mandarins, humbler and more serious about the writings of the missionaries in our study of modern Chinese history, for after all they were first-hand observers and spent most of their lives in China.

The present work is indeed a landmark in the study of American missionary work in China. Fairbank has edited the book with great imagina-

tion and resourcefulness, and his introduction is succinct and masterful.

YU-MING SHAW
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PHILIP C. HUANG. *Liang Ch'i-ch'ao and Modern Chinese Liberalism*. (Publications on Asia of the Institute for Comparative and Foreign Area Studies, number 22.) Seattle: University of Washington Press. 1972. Pp. ix, 231. \$9.50.

Since 1953, five books have been written about Liang (1873-1929). Huang's monograph is the least successful, adding nothing of theoretical interest and little of factual import to the other four. It recognizes and explores briefly a critical development in the growth of Liang's unsteady political philosophy around 1902-03, namely his shift from a focus on individualism to statism. This book also provides some insight into the Japanese intellectual context with which Liang interacted and for which he was a conduit to Chinese readers of his popular journalism. Huang portrays Liang's interpretive translations of Japanese materials and his education into German *Staatsrecht* via Japanese intermediaries in an interesting fashion.

Fortunately, another book focusing with great skill on these issues appeared at the same time as Huang's. Chang Hao's *Liang Ch'i-ch'ao and Intellectual Transition in China, 1890-1907* (Harvard, 1971) does a superb job of explicating Liang's persistent concern with associative values and his key political tract, *The New Citizen*. This book also gives great insight, lacking in Huang's, into the traditional linkages of Liang's political thought, especially the school of "practical statesmanship." Chang Hao's intelligent treatment of Liang's uses of British utilitarianism and Social Darwinism likewise finds no counterpart in Huang's sometimes confused and superficial discussions.

Huang uses the term liberalism indiscriminately, with no more than a casual association with democracy or "individual rights" to specify the term in the first third of the book. Implicitly, in the remainder of the book, utilitarianism, libertarianism, philosophic radicalism, and democracy all jostle each other in the vagaries of his use of liberalism. There is little cooperation between his book's narrative, its subject (Liang and liberalism), and its apparent argument (that Liang abandoned liberalism, understood as individualism, as soon as he embraced it). The *coup de grâce* is the description of Liang's purported reaffirmation of his so-called liberal faith (pp. 147-49). While we are again trying to decode "liberal" (individualist?), the author tells us that Liang has shifted to a belief in cooperation, and he then concludes that Liang has come to a "sub-

stantially different liberal faith." It is unfortunate that the author's useful insights and often imaginative research were saddled with the dead weight of ill-considered terminology.

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ALBERT FEUERWERKER. *Rebellion in Nineteenth-Century China*. (Michigan Papers in Chinese Studies, number 21.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. vii, 101. \$3.00.

ELLIS JOFFE. *Between Two Plenums: China's Intraleadership Conflict, 1959-1962*. (Michigan Papers in Chinese Studies, number 22.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. x, 72. \$3.00.

S. BERNARD THOMAS. "Proletarian Hegemony" in the Chinese Revolution and the Canton Commune of 1927. (Michigan Papers in Chinese Studies, number 23.) Ann Arbor: The University of Michigan Center for Chinese Studies. 1975. Pp. x, 187. \$3.00.

A common thread—the strivings of rebels and revolutionaries in retreat and in defeat—connects these three short monographs from the University of Michigan. Albert Feuerwerker's seasoned study treats a century of dissidence at various levels of intensity and menace to the Ch'ing dynasty. Ellis Joffe, with impressive scholarship, ruminates on various interpretations of Cultural Revolution materials that cover the power struggle during four uniquely discouraging years. S. Bernard Thomas conscientiously examines the Comintern's weak adventure in China, the disastrous Canton Commune of December 11-13, 1927.

Feuerwerker, in his one-hundred-page study, attempts "to describe and analyze as one broad social phenomenon . . . a remarkable number and variety of unsuccessful challenges to the Manchu regime" between 1796 and 1895. Following a skillful retelling, apparently from standard sources, of the stories of various uprisings, but emphasizing the Taiping Rebellion, he examines sources of social dissidence under three headings: social strains, various ideologies of rebellion, and the discontent of rebellious social groupings. He presents a valuable classification of ascending levels of rebelliousness in "a hierarchy of dissidence"—"ad hoc local riots; lineage and communal feuds . . . ; religious and political underworld [activity] . . . ; politicalized bandits; rebellion . . . ; and unqualified political and social revolution."

The study ends with a page of reading suggestions, listing twelve contemporary works by ten authorities. Both specialists and teachers will miss intratextual reference to these authorities, along

with critical footnotes. Feuerwerker's ideas overflow and his style is smooth. The book is interesting and provokes reflection. His analysis highlights the fact that dissidence usually vanishes quickly when it goes unchanneled by purposeful leadership.

Joffe's purpose in *Between Two Plenums* is to isolate threads of historical continuity that stretch directly to the Cultural Revolution, in the context of power struggle. Thus, the collapse of the Great Leap Forward was the event that shattered intra-leadership consensus and opened the period under discussion. The public disputes of 1962, eventuating in Mao Tse-tung's return in formal triumph with the nominal support of a united leadership at the September 1962 Tenth Plenum of the Eighth Central Committee, ended the period. Between these points Joffe discusses the failure of P'eng Tehuai's 1959 challenge to Mao and the purge of P'eng, subsequent political churning and leadership retreat, intellectual dissidence in Peking, continuing submerged controversy over what to do, and Mao's retreat and return.

Joffe does not orient his readers with respect to the PLA and its politicization by Lin Piao, contemporary strategic and nuclear matters, the Sino-Soviet quarrel, Chou En-lai's crumbling diplomacy of those years, the faltering economy as a whole, the ongoing planning crisis, or the significance of the dissidence of the Peking party committee dominated by P'eng Chen, then a formidable figure. It is fortunate that pages 344-61 of Joffe's chapter in the Lindbeck volume ("The Chinese Army Under Lin Piao: Prelude to Political Intervention," in John M. H. Lindbeck, ed., *China: Management of a Revolutionary Society* [1971], pp. 343-74) cover in succinct and usable fashion most of the ground of the present fifty-eight-page study and can be recommended for his views on PLA involvement at that time.

S. Bernard Thomas, in an ideologically directed study of 102 pages with 84 pages of notes, analyzes the outcome of the Canton commune failure for urban-oriented Communists. He takes up the 1928-31 Comintern and Chinese Communist party evaluations, the later appearance of the proletarian hegemony theme in the Kiangsi Soviet from 1931 on, "Maoist proletarian hegemony" in the war years, and, after 1949, Maoist ideological, class, and developmental patterns in relation to the "new symbolism" of Canton.

Thomas' conclusions begin with the statement that "the Canton Commune, both as revolutionary history and political symbol, has apparently run its full course in China to its present status as a virtual nonevent" (p. 175). However, his own work shows it was more than that: the symbolic content metamorphosed over time, and the original signifi-

cance of the proletarian hegemony theme against the backdrop of an old-fashioned semicolonial peasant society has simply lost its relevance in the current phase of socialist construction in China.

The ideological analysis deserves more minute evaluation than I can provide. However, Thomas has clearly traced a stream of ink, if not of causation, from Canton to Peking in 1966, when radical democracy was again briefly in vogue. If only Thomas had discussed the 1966 and 1971 propaganda themes of the Paris Commune against the backdrop of his study, then the most intriguing current question stimulated by his work might have been satisfied.

PAUL ELMQUIST
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WINSTON HSIEH. *Chinese Historiography on the Revolution of 1911: A Critical Survey and a Selected Bibliography*. (Hoover Institution Studies, 34.) Stanford: Hoover Institution Press, Stanford University. 1975. Pp. xvi, 165. \$9.00.

This short book is an important contribution to the growing Western literature on the republican revolution of 1911. It consists of two unequal parts. The first is an extended essay highlighting "the representative works and the major trends of Chinese historical writings on the Revolution" (p. 14). In it Hsieh deplores the narrow focus through which both Nationalist and Communist writers have heretofore viewed the revolution, particularly their excessive preoccupation with Sun Yat-sen and his revolutionary organizations. It is an outstanding historiographical survey—independent-minded, critical, comprehensive, yet concise—but aimed perhaps more at the working scholar than the beginning student. Some of the references, for example to Sun's disavowal of parts of his book *Kidnapped in London* (p. 16) and Ch'ü Ch'iu-pai's attacks on P'eng Shu-chih's social analysis (p. 44), deserve more of an explanation than they are given.

The second part of the book is a list of 368 selected Chinese titles, articles as well as books, for which the preceding essay serves as the annotation. This list, which is by no means exhaustive, consists mostly of secondary works and compilations of primary materials but, generally, not the primary sources themselves. The utility of the list is greatly enhanced by the inclusion of the call number of each item at the East Asian Collection of the Hoover Institution, which has all but about twenty of the works listed. One wishes that the location of the remaining items had also been indicated, since they appear to be quite rare. One wishes, too, that the book had been proofread with

greater care to catch the excessive number of missing characters and mistaken transliterations.

The author of a recent Harvard dissertation on the 1911 revolution and the coeditor of the Chinese volume of the Skinner bibliography on modern Chinese society, Winston Hsieh brings to this project unparalleled credentials. It is thus to be regretted that, for some unexplained reason, the book, though completed in 1972, was not published until 1975.

EDWARD RHOADS
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DIANA LARY. *Region and Nation: The Kwangsi Clique in Chinese Politics, 1925-1937*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1975. Pp. x, 276. \$19.50.

The relationship between regionalism and nationalism is one of the most difficult questions facing the historian of modern China. To what extent was China becoming, in the words of St. Augustine, "an assemblage of reasonable beings bound together by a common agreement as to the objects of their love," and to what extent was the primary object of their love the nation rather than the region, the village, the clan, or the family? Diana Lary deals with these questions forcefully and directly. She argues that the leaders of Kwangsi province, the Kwangsi Clique, were both regionalist and nationalist. They believed in a kind of "layered nationalism" in which regionalism in the present was a means to achieve nationalism in the future. The author quite correctly points out, however, that the persistence of regionalism undercut national unity. The Kwangsi Clique sought to escape their regionalist identity by becoming part of a nationalist movement; in the process they became more, rather than less, regionalist and in control of a larger region.

Lary has performed a number of difficult tasks well. First, she has taken a single province, Kwangsi, and placed it in the context of national politics from 1925 to 1937. As a provincial study, *Region and Nation* underlines the precariousness of KMT power in the border provinces. Second, she provides an excellent discussion of the second level of the Kwangsi Clique. In addition to the four leaders—Li Tsung-jen, Huang Shao-hsiung, Pai Ch'ung-hsi, and Li Chi-shen—the second level provided a core of men indispensable to the survival of the clique. Why was the Kwangsi clique unusually cohesive? The answer lies in the strong sense of provincial identity at the second level, in military service patterns which encouraged second-level leaders to remain in Kwangsi, in "a com-

munity of interest" between the first and second levels, and in the fact that no prominent individuals emerged from the second level to challenge the leaders for supremacy. Third, she provides a lucid account of the Kwangsi Reconstruction Movement and contrasts it with the National Reconstruction Movement. Unlike national leaders, the Kwangsi Clique was aware that reconstruction has to start in rural, not urban, areas, and had to penetrate to the village level. For all their efforts in reconstruction, however, Kwangsi leaders ultimately lacked the determination to effect thorough social change.

Region and Nation might well serve as a model for other writers of provincial history. Lary has both grounded her work in careful research and exercised her sense of historical imagination to illuminate the ways in which provinces relate to the Chinese nation.

DAVID DEAL
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T. A. BISSE. *Yenan in June 1937: Talks with the Communist Leaders*. (China Research Monographs, number 11.) Berkeley: Center for Chinese Studies, University of California. 1973. Pp. 71. \$5.00.

DEREK J. WALLER. *The Kiangsi Soviet Republic: Mao and the National Congresses of 1931 and 1934*. (China Research Monographs, number 10.) Berkeley: Center for Chinese Studies, University of California. 1972. Pp. 116. \$5.00.

The studies under review deal with two of the most critical periods in the history of the Chinese Communist movement. They do so in completely contrasting styles.

Derek Waller's *The Kiangsi Soviet Republic: Mao and the National Congresses of 1931 and 1934* is a careful study of the politics of the formative meeting of the Central Soviet government created in Kiangsi in November 1931 and of its Second National Congress in January 1934, held only a half-year before the beginning of the legendary Long March to the north.

The addition of Mao's name to the drier title of Waller's 1968 University of London dissertation on the same subject is ironic, given his conclusion that the 1931 and 1934 congresses, "far from being stepping stones on Mao's road to power, were in reality evidence of his temporary decline" (p. 114). The group known as the "Russian Returned Students" or "28 Bolsheviks" had replaced Mao and many associates from most positions of authority by early 1934, but the disasters of succeeding months and the early stages of the Long March subsequently precipitated the definitive ascendancy of Mao Tse-tung as the pre-eminent Communist leader. It is also ironic that the writer gives so

little attention to the elaboration of the governmental apparatus these congresses set up, since work along these lines, as Ilpyong Kim and others have shown, permitted the development of the principles of the "mass line" and other techniques that would carry the Communists to victory in the 1940s, even if they could not stave off Nationalist attacks in 1934.

Waller's study, however, gives sound evaluations of Chinese Communist party politics and struggles of the period from 1930 to 1934, and it is the authoritative work on the government congresses themselves. One hopes for future work by the author and others on the localities represented at the National Congresses not only in Kiangsi but in other areas where the Communists managed to survive against enormous odds in the early 1930s.

The appeal of T. A. Bisson's *Yenan in June 1937: Talks with the Communist Leaders* is quite different. It offers the vicarious thrill of a trip by train taken by the author and three other Westerners from Peking to Sian, and then by a rickety car over flooded streams and roads to the Communists' new capital of Yenan. The adventures of this trip and the return journey to Sian, the continued breakdowns of the car and other hazards of the road, and the ingenuity of the driver (a son of Scandinavian missionaries long resident in North China) in dealing with these problems are constantly diverting.

The other part of the book consists of reports, recorded from Bisson's preserved notes, of interviews with Army Commander Chu Teh, a leader of the "Russian Returned Students," Ch'in Pang-hsien (Po Ku), Chou En-lai, and Mao Tse-tung. At the time they took place, just two weeks before the Marco Polo Bridge Incident of July 7, 1937 (which began World War II in China), these interviews would have illuminated the Chinese Communist movement, but thanks to the work of Edgar Snow, then writing *Red Star over China*, of Nym Wales, and of many others, there is little new in them now.

Yet, in the wake of the death of Chou En-lai, these interviews are further proof of the brilliance and confidence of the top leaders, who correctly foresaw the imminence of war with Japan and who had devised a tactic that would ensure the growth of their power, while temporarily subordinating themselves to the Nationalists in a new united front against Japan. This point contains several erroneous statements, perhaps based on what Bisson was told at the time—that Chang Kuo-t'ao, who would be expelled from the Chinese Communist party in April 1938, opposed formation of the second united front with the Nationalists as a betrayal of Communist principles. If anything, Chang was expelled, as he later said, for the opposite reason—that he accused Mao Tse-tung of not

making enough concessions to the Nationalists and of being interested only in expansion at the expense of both the Nationalists and the Japanese. Finally, the excellent and little-known photographs of the Communist leaders and various scenes of the trip are alone sufficient cause for the publication of this book.

JAMES P. HARRISON
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DAVID M. LAMPTON. *Health, Conflict and the Chinese Political System*. (Michigan Papers in Chinese Studies, number 18.) Ann Arbor: Center for Chinese Studies, University of Michigan. 1974. Pp. xii, 149. \$3.00.

David Lampton's theme in his study of the politics of health care in the People's Republic of China is simple: "Scarce resources must be distributed. In a socialist society, there is no entirely satisfactory way to accomplish this. As a consequence, policies have generally reflected the prevailing range of pressures, many of which are beyond the elite's control" (p. 17). Such a hypothesis, even though it seems only common sense, needs to be stated and demonstrated. Too many Westerners have taken statements of goals in the Chinese media for statements of accomplishments. Too many writers have portrayed a monolithic China run by a few decisionmakers; or, with the revelations of factionalism during the past decade, scholars have sought one overall model of political conflict in China since 1949.

Lampton starts with the assumption that Chinese leaders wish to make the vital health services available to all citizens; he then analyzes the implementation of this goal and illustrates the limitations on policy alternatives and the consequences thereof. For example, the problem of leader-mass linkage became acute during the Great Leap Forward because middle-level cadres heading the communes had few ties with either the local populace or the professional organizations. The cadres were thus not always responsive to local health concerns, and they lacked the professional contacts to appreciate some of the medical consequences of their policies. In this vein, Lampton points out that the proliferation of barefoot doctors resulted in a multiplication of referrals to urban hospitals and thus overburdened the professional physicians and the medical facilities. Under the category of fragmentation of authority Lampton discusses three health policy-making arenas: the Ministry of Public Health, the Nine Man Subcommittee on Schistosomiasis, and Commune Party Committees. The policies of the three differed because of dissimilar responsibilities and also

because of variations in their resources, perceptions, and values.

Despite conflicts and limitations and a succession of policy-making systems that made no one model adequate, Lampton concludes that the health care bureaucracy has embodied a relatively broad range of social interests. He notes that the problems under discussion are characteristic of a developing medical system, and this in itself is evidence of significant progress in the delivery of health care.

One criticism must be made: the work is composed of three separate essays, and the result is considerable repetition. This duplication, combined with Lampton's penchant for summarizing each section, leads the reader to a feeling of *déjà vu* by the time he reaches the third essay.

JESSIE G. LUTZ
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CYRIL E. BLACK *et al.* *The Modernization of Japan and Russia: A Comparative Study*. New York: Free Press. 1975. Pp. xiv, 386. \$15.95.

For many years one of the most satisfying seminars in my course on the world economy has been the comparison of modern economic growth in Japan and Russia. I opened this book, therefore, with some excitement, and I was not disappointed when I closed it. What distinguishes this study, aside from the expertise of the eight authors, is their achievement, after protracted exchanges, of an agreed structure. This is not a loosely coordinated group of learned essays. It is a quite coherent book. Modernization is defined as both the capacity to accept progressively the fruits of modern science and technology and the societal changes wrought by that process.

The analysis proceeds along a horizontal and a vertical axis: three stages of modernization take us through time; five criteria permit systematic cross-sectional comparison of the two societies for each stage. The three stages are: the premodern past as it contributed to (or frustrated) the preconditions for modernization; the transformation, embracing, in my vocabulary, both the take-off and the drive to technological maturity; and "high modernization," when the full range of modern technologies have been more or less efficiently absorbed and, in one degree or another, the mature industrial system provides the gadgetry (as well as services) of mass consumption.

The most original portion of the analysis covers the first stage. It is argued at length and with subtlety that Japanese and Russian history had, over preceding centuries, prepared their societies rather well to react positively to the shocks of Commodore Perry and the Crimean War. Prior

centuries of effective national government and the experience of adapting much from China and Byzantium, respectively, are particularly emphasized.

Similarities and divergences in the transformation and high modernization emerge clearly, although more might have been made of the differences in pace and character of nineteenth-century land reform, and I found the economic comparisons excessively over-aggregated. Moreover, the stage of transformation is a bit too long and complex to be dealt with comfortably in a single sweep.

The greatest puzzlement, for authors and readers, is the question of the general lessons that can legitimately be drawn from the story of the modernization of these two remarkable nations. The authors are, properly, uneasy and indecisive as they look over their shoulders and comment on China's delay of almost a century in entering the transformation. And the broad concluding lessons toward which they lean (without dogmatism) might have been modified if they had considered some other nations which entered the transformation at about the same time as Japan and Russia, for example, Sweden, Italy, Hungary, and Canada.

But on balance this is a model in comparative historical analysis which ought soon to be available for students in paperback.

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Austin

HERMAN OOMS. *Charismatic Bureaucrat: A Political Biography of Matsudaira Sadanobu, 1758-1829*. Chicago: University of Chicago Press. 1975. Pp. xiii, 225. \$10.50.

We now have scholarly English-language monographs on the two powerful ministers who in turn dominated Japan's shogunal government in the last four decades of the eighteenth century: John Whitney Hall's book on the corrupt but innovative *Tanuma Okitsugu (1719-1788): Forerunner of Modern Japan* (1955), and Herman Ooms' new work on Matsudaira Sadanobu, who purged Tanuma's supporters, reversed his economic policies, enforced an austere Confucian moral code, and banned heterodox teachings in the schools.

Ooms argues that the last two of these actions made Matsudaira a "charismatic" bureaucrat who was "the first conscious creator of an 'ideology' for Japan." He justifies this description by redefining "charismatic" so that "a leader can be charismatic through the particular self-perception he has of his own role," without attracting "mass followings." It is much easier, however, to accept what Ooms rejects—Nakane Chie's observation that Japanese

history shows "a conspicuous absence of charismatic leaders" and a conspicuous preference for collective leadership and decision-making.

The strong points in Ooms' account are the sections describing Matsudaira's copious writings, his ethics, his political ideology, and his economic measures. All of these improve our understanding not only of the man but of Japan in his era. However, the sections on political history have several lacunae. Ooms discovered an astonishing document itemizing bribes paid by Matsudaira to Tanuma for an appointment which enabled him to plot Tanuma's downfall. This new evidence tends to complicate, rather than answer, questions Hall raised about that appointment. Ooms does not discuss Hall's questions, or explain why Matsudaira's sister opposed his next appointment as chief councillor.

Nor is there discussion of Ooms' implicit disagreement with Conrad Totman's portrayal (in *Politics in the Tokugawa Bakufu* [1967]) of Matsudaira as more the agent than the principal in subsequent maneuvers, but an agent who soon became unresponsive to his backers' wishes. Similarly, in an otherwise admirably detailed account of the constitutional crisis over an imperial title, we are not told why the imperial regent opposed his own brother and his nephew, Emperor Kōkaku, in order to side with Matsudaira. Ooms argues that Matsudaira's downfall was due more to the Tokugawa 'Three Houses' displeasure over this "stern treatment of the imperial court" than to resentment over the sumptuary laws. But he says so little about the motives of the Three Houses or the impact of the sumptuary laws that the conclusion seems inadequately documented.

ROBERT M. SPAULDING
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IVAN PARKER HALL. *Mori Arinori*. (Harvard East Asian Series, number 68.) Cambridge, Mass.: Harvard University Press, 1973. Pp. x, 335. \$19.00.

This is a surprisingly good book—surprising because biographies of Japanese leaders rarely succeed. Not the least among problems that a biographer frequently encounters is a lack of material on the personal life of his subject, an absence of diaries and personal correspondence that reveal the private thoughts of the person and the sources of his behavior. Ivan Parker Hall encountered this problem, too. "Data relating to Mori's personal life," he admits at the outset, are "particularly thin and disappointing." To a considerable extent Hall compensates for this lack by making imaginative use of Mori's public writings, by providing new and important details of the institutions Mori wanted to reform, and by relying on Mori's flam-

boyance as a public official to give color and interest to the biography.

The background of Mori Arinori (1847–89) sharply contrasted with that of other Meiji leaders. While they were plotting the overthrow of the Tokugawa he was a world apart, ensconced in a Swedenborgian community in upstate New York. He returned to Japan after the Restoration, and, using his knowledge of the West, his language skills, and his Satsuma connections, he gained important positions in the government for the next two decades until his assassination. Among the pragmatic, free-wheeling new leaders he was as innovative as anyone. He founded the first modern philosophic society and the first commercial college, engaged in a Western-style contract marriage, urged adoption of English as the national language, and was the first to suggest abolition of sword-wearing. Mori spent nearly a quarter of his life abroad, including service as Japan's first diplomatic representative in Washington (1871–73) and later as minister at the Court of St. James (1880–84).

Hall provides fascinating accounts of these episodes, but he saves his most detailed treatment for Mori's role as minister of education (1885–89), ascribing to him more influence in this position than any of his successors. The author displays here a refined understanding of the statist and elitist policies that Mori instituted and of their origin in his personality and in contemporary education theory in Europe. In places one may want to cry out at the prolixity of Hall's account, but if the reader will bear with the superabundance of detail he will be rewarded with much important information on the Meiji education system. Hall supplements Japanese scholarship with exhaustive research in Western archives and shows the powerful influence that contact with individual Westerners, especially Herbert Spencer, exercised over Mori's ideas and subsequent policies.

In its meticulous analysis of Mori's utilitarian approach to Western culture, this book makes a substantial addition to our literature on the cultural revolution of early Meiji.

KENNETH B. PYLE
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JOHN W. DOWER, edited with an introduction by. *Origins of the Modern Japanese State: Selected Writings of E. H. Norman*. (The Pantheon Asia Library.) New York: Pantheon Books, 1975. Pp. vii, 497. \$15.95.

E. H. Norman became one of the giants of Japanese studies in the West in 1940 upon publication of his classic *Japan's Emergence as a Modern State*. Two monographs followed on the origins of con-

scription in Japan and on the eccentric eighteenth-century utopian thinker Andō Shōeki. The main part of Norman's career took place in the Canadian diplomatic service, not academia. He was ambassador to Egypt at the time of his death by suicide in 1957, a matter generally assumed to be directly linked to allegations made in United States Senate committee hearings that as a student in 1938 Norman had been a Communist.

The core of this book is a long-needed reprint of *Japan's Emergence*. There are also two other of his works that have not previously appeared in English. One is a brief preface, "The Shrine of Clio," written in 1955 for a Japanese-language edition of his essays. It is a gracious and personal statement of his view of history. The other, "Feudal Background of Japanese Politics," was a book-length paper prepared for the Ninth Conference of the International Secretariat of the Institute of Pacific Relations in January 1945. If it had been published at that time it would have been a real contribution to the English literature, particularly on the character of Tokugawa feudalism. Now its interest is mostly historiographic. It is an excellent example of that bleak view of the Japanese past—and for that matter the Japanese present—that a later generation of revisionist historians in Japan and the West brightened up in order to account for Japan's spectacularly successful modernization.

Perhaps the brightening has gone too far. John Dower's introduction, which is the most valuable part of the book, is a long, eloquent essay on the relation between Norman's work and that of his successors among American historians of modern Japan. His charge against the latter is severe: in contrast with Norman's humane concern for the social inequality, authoritarianism, and oppression of modern Japan, they see a past that was not so unbearable after all, moving toward a present and future full of promise. The specific target for criticism is the prevailing point of view of the authors in Princeton University Press' *Studies in the Modernization of Japan* series, a product of the early 1960s when the ongoing dynamic of Japan's social and political development was such as to permit reasonable men to take a positive view of the long-range progress of Japanese history. Dower's critique is to a considerable extent a reflection of the more sober 1970s, when that dynamic seems to have taken a turn for the worse. All seek to explain a particular present in terms of elements selected from history, as Norman had done in the desperate times before and during World War II. Dower does handsomely by Norman in showing and honoring the humanity and depth of his scholarship, but he does ill by the modernization theorists of the 1960s, for their correction of overly negative appraisal of Japanese tradition by a pre-

vious generation of historians was as necessary as Dower's correction of their too cheerful one.

HERSCHEL WEBB
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MARK R. PEATTIE. *Ishiwara Kanji and Japan's Confrontation with the West*. Princeton: Princeton University Press. 1975. Pp. xix, 430. \$16.50.

This is the first and, I hope, not the last major biography of a significant Japanese military figure of the 1930s. Ishiwara's career and thought were readily accessible to the historian through a literary output more prolific than is usual with professional military men. Ishiwara was first a theorist, a General Staff Headquarters officer who devised the strategy of the Manchurian operation, developed the dialectic of an apocalyptic Final War, and fostered the organization of the East Asia League. His own tragedy was that, as the author notes, all of his ideas miscarried. Ishiwara's career intersected with three major series of events of the 'thirties: the "Manchurian Incident," the Young Officers' Revolt, and the China war, and in this respect an appreciation of the man is central to the understanding of the 'thirties. Ishiwara's theories were an anomalous amalgam of sophisticated military analysis and mystical revelation, in part derived from his Nichiren faith. His theory of a Final War rested on questionable historical assumptions based on only a slender body of information about his hypothetical enemy, the United States, which he never took the trouble to visit.

In his mild revisionism of the interpretation of Ishiwara as critic of Japan's despotism in China, Mark R. Peattie relies on the work and views of Hata Ikuhiko, an eminent military historian. On the *gekokujo* question, too, Peattie sides neither clearly with the traditional interpreters of Manchuria as the major instance of that phenomenon nor with the revisionists who emphasize the command principle, *dokudan senkō*. In fact, his mention of this controversy seems more an afterthought brought into his conclusions than a carefully developed theme. He contributes an important corrective to John Hunter Boyle's misreading of Ishiwara's hypothetical chief enemy as the Soviet Union. Peattie correctly identifies the antagonist of Ishiwara's Armageddon as the United States.

Without taking the author to task for not dealing with matters he may not have intended to settle, it would nonetheless have been relevant had he devoted some attention, for example, to the system of Imperial Army education of which Ishiwara was a product, and the process of military decision-making that in many instances rejected Ishiwara's theories. The reader might have hoped, too, for a sharper delineation of the differences that sepa-

rated Ishiwara and Itagaki, on the one hand, from General Staff Headquarters superiors, on the other. We might have anticipated, too, that in 374 pages of text the author might have devoted more than a scant 25 pages to the setting and system within which Ishiwara operated—the world of the Imperial Japanese Army. We must at the same time gratefully acknowledge our debt to the author for contributing a well-researched and welcome biography of a fascinating military figure central to Japan's China venture.

JOYCE C. LEBRA
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R. N. SALETORÉ. *Early Indian Economic History*. Totowa, N.J.: Rowman and Littlefield. 1975. Pp. xv, 859. \$27.50.

This compendium of economic practices and ideas covers two millennia. R. N. Saletore examines a vast body of literary, epigraphical, and numismatic sources and organizes these into chapters devoted to the general structure of external trade, composition and direction of imports and exports, sea and land routes, markets and state control, agriculture, "guilds," and currency and banking. Since a major focus of the volume is on foreign trade, many of the sources are foreign—from the classical West and from China—and some effort is made to compare economic arrangements in all three civilizations. For the field of Indian history, this large work is a unique contribution, valuable especially for its scope, its often erudite judgments on both foreign and indigenous evidence, and for its well-ordered format. To the wider field of ancient economic history—Asian and Western—it provides the most convenient inventory of early Indian economic thought and action.

The volume is a magnificent example of nineteenth-century taxonomic scholarship with the strengths and weaknesses of that genre. There is even something of a nineteenth-century quality in the author. Saletore followed a brilliant university career by an equally successful career as an Indian civil servant (IAS) during which time he published important research on early economic history. He also was a frequent contributor on this subject in the popular English-language press of India. In this he resembles the scholarly administrators of colonial India, whose Orientalist enthusiasms resulted in the critically edited Sanskrit series that comprise the central core of evidence in the present work and whose published research of the late nineteenth and early twentieth centuries comprised a major part of Saletore's bibliography. In Saletore's work, as in that of his British predecessors, the cataloging of traits dominates analysis.

The subject matter of each chapter is organized according to the time period of the specific classes of texts used. This textual periodization is questionable in some particulars (as when early Vedic is given as 2500–1500 B.C.); in general, this periodization has no relationship to phases in the development of the economic order. Saletore gives little attention to the relationship of specific economic arrangements to the larger society or to the causes of changes in these arrangements.

Therefore, as an analysis of the economy of ancient and classical India, this is inadequate. The author completely ignores the work of the Allchins and others on the Neolithic foundations of peasant agriculture in peninsular India and the imaginative scholarship of D. D. Kosambi. Also, the legacy of the Harappan culture (or Indus civilization) is barely mentioned in a two-page discussion of material elements suggesting significant contact between the archaic urban civilizations of Sumeria and India. The author makes no effort to clarify the relationship of early Indian monarchical states and the economic order, a subject that has long been neglected; in the examination of ancient "guilds" (*śreṇī*) as corporative institutions, he fails to address large questions pertaining to sastric conceptions about caste with its crucial occupational concern. In failing to consider these and other fundamental issues, the volume yields less than it should, considering the effort and erudition it represents.

BURTON STEIN
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JAMES MILL. *The History of British India*. Abridged and with an introduction by WILLIAM THOMAS. (Classics of British Historical Literature.) Chicago: University of Chicago Press. 1975. Pp. xlvii, 599. \$20.00.

This abridgement presents James Mill's essential philosophy to general readers and students who might shrink from the full six-volume version. That is its basic merit.

Given the inherent limitations on any abridgement, this one is well conceived. William Thomas (student and tutor in modern history at Christ Church, Oxford) begins with an informative introductory analysis. He provides background and interpretive guidance for casual readers and scholars and shows his own perceptions of Mill, his philosophy and work, and the context in which he operated. This is followed by the editor's equally valuable bibliographical commentary. Finally, Thomas' selections from the formidable *History* are judicious and reasonably balanced. Believing that Mill's central purpose was to demonstrate through his *History* "what he regarded as the most impor-

tant social, economic, and moral thought of his time." Thomas makes his selections to show Mill's general philosophy, omitting lengthy narratives of military campaigns and much of the early history of British activities in India. Ten chapters detail Mill's harsh analysis of Indian culture which he characterized as ignorant, depraved, and backward and, by Utilitarian standards, generally defective. Five chapters represent Mill's philosophic indictment of British domestic institutions and especially of the East India Company's rule in India, chapters in which he offered Utilitarian measures (and Utilitarian Englishmen) to transform India and ultimately England.

For scholars and students of cross-cultural relations, Mill's *History* is an important source, and, on another level, Thomas' abridgement should also be useful. Mill's ideas helped to reverse late eighteenth-century trends toward a modicum of understanding and accommodation between British and Indian cultures and to invoke new British attitudes and policies, characterized by incredible cultural arrogance. These developments produced manifold and often shattering consequences for both India and Britain. For historians and others concerned with cultural interaction, the implications of such developments are compelling.

Students of British intellectual history, British Indian policy, and British perceptions of India in the early nineteenth century should find Thomas' abridgement similarly useful and convenient. For those who want more, Chelsea House Publishers' excellent 1968 reprint provides easy access to Mill's *History* in its entirety.

EDWARD B. JONES
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R. E. M. IRVING. *The First Indochina War: French and American Policy, 1945-54*. London: Croom Helm; distrib. by Crane, Russak and Company, New York. 1975. Pp. 169. \$11.50.

ROBERT F. TURNER. *Vietnamese Communism: Its Origins and Development*. (Hoover Institution Publications, number 143.) Stanford: Hoover Institution Press, Stanford University. 1975. Pp. xxix, 517. \$14.95.

VO NGUYEN GIAP. *Unforgettable Months and Years (Nhưng Nam Tháng Khong The Nao Quen)*. Translated and with an introduction by MAI VAN ELLIOTT. (Southeast Asia Program. Data Paper, number 99.) Ithaca: Cornell University Southeast Asia Program. 1975. Pp. viii, 103. \$6.50.

These three volumes provide a valuable though incomplete story of events in Vietnam since World War II. All three have their own points of emphasis as well as shortcomings, but each adds significantly to the ever-increasing literature on the sub-

ject. And while the Vo Nguyen Giap volume might be dismissed as a poorly written propaganda piece, both the R. E. M. Irving and Robert F. Turner books are quality additions in a field where, unfortunately, emotion, journalistic sensationalism, and quantity at any price have long outstripped solid, objective scholarship. This is the real contribution of both these books, and thus I believe they deserve to be classed with some of the works of the late Bernard Fall and Ellen Hammer.

Irving sets out to answer three questions in his book: To what extent and in what ways did French political parties influence Indochina policy? What were the motives behind the policies pursued by France, the United States, and Vietnam in the period 1945-54? In what ways were these three countries affected, both at the time and subsequently by the process of decolonizing Indochina? Irving succeeds admirably in the first, does an adequate job with the second, and covers the third sufficiently with the exception of the United States. In fact, the United States operates primarily on the periphery as an influence on French politics. Here the subtitle of the book, "French and American Policy, 1945-54," does not hold up. Regardless of what the book purports to be, it is actually a political history of French actions in Indochina and an analysis of the impact of those actions on the French political scene, focusing primarily on the Christian Democratic party. In his consideration of that party, Irving is superb as he traces the vacillations in French policy toward Indochina and identifies those key points when the Indochina problem might have been alleviated. He also sorts out the motivations for French policy and presents them clearly and meaningfully. He brings order out of the chaotic French political scene and makes his most valuable contribution by putting Indochina policy into perspective vis-à-vis French political chaos. He has not, however, written about the first Indochina war and has actually plowed little new ground. He ignores the military situation except as it bears directly upon French politics and draws the United States ever closer to active intervention—a policy to which he believes the U.S. was committed even prior to the 1960s.

Turner's volume is certainly one of the most refreshing to appear in several years on the subject of Vietnam, and it rekindles confidence that there is quality work again appearing after an interlude of emotional and severely biased pieces. In fact, Turner's work must rank as a landmark in the treatment of Vietnamese communism. The Communist party that emerges from his pages is not that of the kindly old Vietnamese nationalist, Ho Chi Minh, or the party that has the complete and absolute support of all the people in North Viet-

nam. Rather it is a fledgling party attempting to consolidate its control over an apathetic and often hostile people in the name of nationalism, using all the tricks in the book to do so. Of particular value is Turner's analysis of the North Vietnamese problems with the Communist bloc, as well as with former enemies. He also finds Chinese influence strong in the political formation of the Vietnamese government and the party much less independent of foreign influence than many authors would lead us to believe. Not only is Turner objective and thorough, he has gone into both Communist and non-Communist sources extensively and produced a carefully researched volume. Included as appendixes are many of the relevant documents needed to obtain a complete picture of the evolution of the Communist regime during its rise to power in North Vietnam. The primary weakness of the volume, as with Irving's, is his almost complete lack of treatment of the military and its role in the rise of Vietnamese communism.

Certainly the least valuable of the three volumes is Giap's *Unforgettable Months and Years*. It bears the clear marks of Giap's writing: poor organization, rambling style, and heavy propaganda. If there were any doubt about its being a propaganda piece, such doubt is rapidly dispelled by reading Mai Van Elliott's introduction, which is also sharply biased toward the objectives and purposes of the North Vietnamese. Yet the Giap book is valuable when its many stories of the trials and tribulations of the kindly "Uncle Ho" struggling to help all his people are contrasted with the stark reality of events as seen by Turner and the tremendous political infighting and indecision portrayed by Irving. One must also note the contribution of Cornell University to the study of Southeast Asia in publishing (so far) over one hundred volumes on all facets of Southeast Asian society, politics, and culture.

PHILIP D. CAINE

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JOHN A. LARKIN. *The Pampangans: Colonial Society in a Philippine Province*. (Publications of the Center for South and Southeast Asia Studies, University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xvii, 340. \$13.50.

It is difficult to review John Larkin's monograph, which has been reviewed adequately in several journals some years ago. The task is even more challenging when I remember telling the author (while we were both in Manila in late 1963) that any historical study of a Philippine province was next to impossible and certainly a waste of a graduate student's time. Today this work stands as a

model for young scholars interested in the Philippines. Its publication proves that regional or provincial research is both possible and rewarding. Most older Filipinista scholars, myself included, were trained to see everything from the perspective of imperial Manila, and we rarely ventured into the provinces or municipalities and *barrios* for any understanding of Philippine history. We tended to stay where the research materials were available without excessive physical or financial burden to ourselves. We chose, in many instances, subjects that would include Spaniards, Americans, or Japanese because these regimes tended to have voluminous records, if not always well arranged, kept in Madrid, Manila, Washington, or Tokyo. No one suggested that maybe the history of a region (other than Mindanao and Sulu because they are the homeland of the Filipino Muslims) or a province might contribute to Philippine historiography. In exculpation, my generation was not trained to look upon local history as necessary or rewarding anywhere, including the Philippines.

Larkin traces Pampangan history from the earliest records through the three centuries of Spanish rule down to the third decade (1921) of the American occupation. He not only treats the usual fare of historians, but he also discusses the socioeconomic aspects of Pampanga's existence. What comes forward from his well-researched study is that Pampanga's problems (the rise of a cash-crop economy, later based heavily upon sugar, the migration of Chinese into the province and their intermarriage with the indigenous population, the growth of socioeconomic discontent among the peasantry, and the dominance of local issues over Manila politics) were not Pampangan alone. But scholars will not know, without studies of many more Philippine provinces, to what extent local priorities counted among the provincial elite or how much discontent among the peasantry was attributable to local conditions over which Manila had no control. Perhaps scholars will conclude that the Philippines, whether under Spain, Washington, or President Marcos, is not a highly centralized state.

The author has been told many times that his work presents a new challenge to Philippine historiography. It is good that he persevered through nearly a decade of research and writing.

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JOHN R. M. TAYLOR, a compilation of documents with notes and introduction by. With an introduction by RENATO CONSTANTINO. *The Philippine Insurrection against the United States*. Volume 1, 1571 to May 19, 1898; volume 2, May 19, 1898 to July 4, 1902;

volume 3, *May 19, 1898 to February 4, 1899*; volume 4, *February 5, 1899 to November 13, 1899*; volume 5, *November 13, 1899 to July 4, 1902*. (Eugenio Lopez Philippine History Series.) Pasay City, Philippines: Eugenio Lopez Foundation. 1971. Pp. xxii, 525; xii, 524; viii, 672, 129; xiv, 809; xviii, 829.

The publication of these volumes is an event of considerable importance to Philippine and American historians. The Eugenio Lopez Foundation in Manila has produced a five-volume limited edition of *The Philippine Insurrection against the United States*. The documents included were selected from the enormous body of official papers of the Philippine Republic, papers seized by the invading American Army between 1899 and 1902. The totality of the papers will always serve as the chief source of information about the young nation, its administration and other affairs, and its relations with its Spanish and American adversaries. For this reason the so-called Philippine Insurrection Records represent a valuable addition to our knowledge of the Philippine-American War, and the foundation of United States imperialism in Asia.

As the war reached its conclusion, cartons of papers were shipped to Washington where Taylor began the monumental task of choosing and translating what he considered the items most germane to an understanding of the events of the preceding years. By 1906 he had completed the job and had arranged thousands of translations into 1,500 exhibits. To this compendium he added his own historical introduction that described the Philippine struggle for independence from Spanish colonial times to the final military suppression of the young republic in 1902. And despite Taylor's obvious tendency to portray the United States in the best possible light, his notes and arrangement still prove a useful way into the documents. When he sought to publish his great work, however, he ran into opposition from Secretary of War William Howard Taft, who feared that it might serve as a source of embarrassment for the Republican administration; hence although the work ultimately reached the stage of a third galley, it never became available to the general public. For years it has remained only on microfilm at a few selected archives in the United States and the Philippines.

To encourage the historical investigation of that era, the Lopez Foundation has printed this edition from the microfilmed galley proofs and has distributed five-volume sets to a number of Island and American libraries and to a select group of Philippine specialists. Chief editor Renato Constantino has drawn upon two of the three galley versions in carefully preparing this edition, and he has meticulously added a list of the errata he and his staff have corrected. Moreover, he has kept the galley

numbers for those who might wish to check the current edition against the proofs. In spite of its length, the work is remarkably free from flaws, except for the misplacement of a small number of exhibits that had to be added to the end of volume 5. The Lopez Foundation certainly merits our gratitude for this fine scholarly effort. Though the set has not been offered for sale, it can now be found in many major university and public libraries.

Finally, scholars should note that these five volumes contain only a minute portion of the captured papers. The printed text came from galleys on nine reels of microfilm; an additional 643 reels of largely unedited, unclassified, and untranslated materials also exist. While Taylor selected judiciously, he tended to favor those documents of national and international importance. Much material pertaining to provincial- and village-level affairs lies buried in the confusion of the unused portions. In recent years the original records have been returned to the Philippines where they are being rearranged at the National Library in Manila. However, given the fact that with each move and each new handling some papers are lost, it is probable that the 643 reels of unsorted papers located in the National Archives in Washington remain the most complete, if the most cumbersome, collection of documents. The Taylor distillation, now more readily available for the first time, provides an excellent introduction to this crucial source.

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KHOO KAY KIM. *The Western Malay States, 1850-1873: The Effects of Commercial Development on Malay Politics*. (East Asian Historical Monographs.) New York: Oxford University Press. 1972. Pp. x, 244. \$15.50.

M. C. RICKLEFS. *Jogjakarta under Sultan Mangkubumi, 1749-1792: A History of the Division of Java*. (School of Oriental and African Studies, University of London. London Oriental Series, volume 30.) New York: Oxford University Press. 1974. Pp. xxv, 463. \$29.00.

WILLIAM R. ROFF, editor. *Kelantan: Religion, Society and Politics in a Malay State*. New York: Oxford University Press. 1974. Pp. xvii, 371. \$25.50.

Southeast Asian historical scholarship since World War II has shifted in focus from the regional to the national to the local. Already more than two decades ago scholars discussed and encouraged this direction. Now the results are here, and they are stimulating and welcome, for not only do they provide sharper insights into the motivating forces

of change, but they also lay a new foundation upon which historians of the future can rebuild national histories.

Khoo Kay Kim, recently designated professor of history at the University of Malaya, has analyzed the political impact of the influx of capital and migrant labor from the Straits Settlements, especially Malacca and Penang, upon the nearby Malay states of Perak, Selangor, and Negri Sembilan on the west coast of the peninsula. In the quarter century before direct British intervention in 1874, this influx proved the undoing of the traditional Malay authority patterns, which were unable to provide either the necessary stability or the mobilization of resources that the new circumstances demanded. In willy-nilly fashion they fell into disarray and confusion. Khoo moves the story forward from one state to another in such detail that the reader may be forgiven for skipping names and details. But the main directions are obvious, for out of confusion there must emerge order—capital investment would not have it otherwise—and the age-old flux of Malay politics between local autonomy and central authority moves in the end to the latter pole.

The book about the east coast Malay state of Kelantan, edited by W. R. Roff of Columbia University, focuses upon a more limited area and concerns a slightly later time period, namely from the 1890s to the present. But the main themes of the story it tells resemble those of the Khoo book. Outside authority, first in the form of Siamese administrative reorganization and after 1909 in the form of British "advisers," proves the undoing of the traditional Malay system, replacing it by something that could probably best be described as a neotraditional Malay system. Twelve authors, including local Kelantanese, have each written a chapter. The story is carried forward both topically and chronologically, and the editor is to be praised for the skillful fashion in which he has integrated all the contributions into a comprehensible whole, for while the book is essentially a history it contains contributions by authors in other disciplines. The overall effect is to bring the reader very close to the local scene and into the institutions and society of Kelantan.

The period from 1892 to 1915 witnessed the same incoherence that occurred earlier in the west coast states as traditional patterns crumbled. The chapters of this book take us through a rebuilding of institutions and a restructuring of society that show historical change as something which is much more than a surrendering of the traditional to the modern. Instead, a complicated competition developed among social groups striving for new relationships with each other while each was variously affected by forces of modernization. Tradi-

tional values were used to different ends: some institutions, such as the Islamic religion, could emerge more organizationally powerful than they ever had been, while others, such as the traditional elite, had their status eroded by new social forces. In the search for meaning and value in life the Kelantanese in some ways politicized a fiction and suppressed reality in trying to uphold Malay ideals. This study of social change will not provide easy answers, but the reader will be rewarded with thought-provoking insights into what modernization means in the context of a Malay society.

In the volume by M. C. Ricklefs, who is currently at the School of Oriental and African Studies in London, we confront a different set of values and circumstances. The breakup of the Javanese empire of Mataram plays on a stage of great tradition that relegates the Malay states discussed by Khoo and Roff to the realm of little communities. Using hitherto untapped Javanese sources and skillfully blending these with coeval Dutch accounts, Ricklefs has drawn a revealing and sympathetic picture of the first sultan of Yogyakarta and the way in which he viewed the events of his time and place. At the heart of this story is the dissolution of a once-great empire. The European presence was undoubtedly involved in this decline, but Ricklefs makes clear how very peripheral this presence was to the Javanese participants. Malay states of the eighteenth century were still masters of their own destiny, or thought they were, and as a consequence dealt with Europeans in a manner quite different from what they were to do later. Because their values were still vital, their written records embody cultural values that are difficult to translate into clear expositions of intent and purpose, but Ricklefs manages this feat with skill and beauty. The Javanese tradition has found its modern historical expression in this volume.

ROBERT VAN NIEL
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J. D. LEGGE. *Sukarno: A Political Biography*. New York: Praeger Publishers. 1972. Pp. ix, 431. \$10.95.

John Legge's temperate and tentative assessment of Sukarno is an accomplishment of the first order in modern Indonesian history, and in Southeast Asian and postcolonial Afro-Asian history as well.

Legge knows Indonesia and the Sukarno record, and he makes sensible and supple use of the scholarly context built up by the students of Feith, Benda, Geertz, Dahm, and Kahin. In addition to monographic material, he uses the Indonesian press, Sukarno's writings and speeches, and interviews with many who knew the Indonesian leader. Legge has not intended to write an exhaustive

biography, but he has asked significant questions and answered them with care and dispassion.

How much of a revolutionary was Sukarno, as distinct from a dynamic rhetorician? How much a unifier was he, as distinct from an accomplished balancer? How much a modernizing statesman was he, as distinct from a Javanese traditionalist? What made him tick? Sound answers require sensitivity to differing conditions and opportunities in the four decades of Sukarno's public career. Legge's interpretation (for example, pp. 14-15, 319-20) emphasizes three principal periods of political creativity. First, in the late 1920s, as the leader of the PNI, Sukarno gave a new direction to Indonesian nationalism. Then came exile. Second, during the Japanese occupation he established himself as the national leader of Indonesia, leading it toward revolution. Then came a figurehead presidency. Third, as the political parties and Constituent Assembly failed their critical tests from 1956 to 1959, Sukarno became the chief architect of "guided democracy."

Legge sees the years of greatest formal power, those after 1959, as ones in which magico-mysticism, along with moral coarsening, prevailed in Sukarno's behavior. He was devoid of substantial thought on how to achieve economic progress or fundamental alteration in the structure of society. The events of September 30-October 1, 1965, shattered the triangle of forces among which he had maintained a manipulative balance.

Legge implies that without the Sukarno of the late 1920s and 1942-45, there would have been a different and much weaker anticolonial revolution. Without the Sukarno of those periods and 1956-59, there would have been a far less successful synthesis of the national idea in a diverse state. In the end, however, the Javanese traditionalist in his makeup outweighed the modernizing statesman. He changed Indonesian history with his syncretic energy and magnetic personality, but his political *mantras* were unequal to the problems of the 1960s.

What made Sukarno run? With this book we can listen closely to the ticking but cannot look inside the case. There will be much to learn if a psychohistorian of the caliber of Erikson, Mazlish, Brodie, or Rogin has the patience to master pre-modern Javanese culture, as well as post-Freudian analytic theory. Meanwhile, we have a first-class political biography of a major leader of the mid-twentieth century.

THEODORE FRIEND
Swarthmore College

R. H. W. REECE. *Aborigines and Colonists: Aborigines and Colonial Society in New South Wales in the 1830s and 1840s*. [Sydney:] Sydney University Press; distrib.

by International Scholarly Book Services, Portland, Ore. 1975. Pp. ix, 254. \$19.80.

DAVID DAVIES. *The Last of the Tasmanians*. New York: Barnes and Noble. 1974. Pp. 284. \$13.50.

W. K. HANCOCK. *Discovering Monaro: A Study of Man's Impact on His Environment*. New York: Cambridge University Press. 1972. Pp. xii, 209. \$12.50.

Sir Keith Hancock has made a highly successful venture into Australian local history. The book is a brilliant study of a six-thousand-square-mile area in the southeast corner of Australia. Composed of grassy upland and alpine watershed, Monaro is one of the few sources of water in the world's driest continent. Investigators usually focus on Monaro's nineteenth-century impact, good or bad, on the Australian environment. Hancock begins with the aborigines, who over many generations adapted the land to their use by burning it off and turning it into grassland. The author speculates, too, on the effects on Monaro of the arrival of animals and plants. Finally, the white man drove the aborigines off the land. Hancock's basic question is, "How has man in Monaro used the land on which he lives" (p. 14)? He calls upon anthropologists, biologists, botanists, geographers, geologists, and other social scientists to speak of the area, and he allows settlers to report for themselves in letters, songs, poetry, and journals on their struggle with the land and the effect the way of life had on them. Each wave of occupants despoiled the land, then were forced to improve it if they were to survive. This book is an object lesson in the use of interdisciplinary studies; it has complexity, variety, and clarity. Through his asides or running commentary, Hancock gives the reader the sense of having a personal tour of this rural area.

R. H. W. Reece's book pioneers in trying to discover the essence of the aboriginal problem in the 1830s and 1840s and the way contemporary philanthropists, squatters, and colonial administrators viewed the problem. Reece admits he is limited by the fact that the only existing documentary material is from white sources, meaning that white perceptions of aborigines dominate the problems of culture contact. He regrets the lack of documentation of aborigines' perceptions. The objective of the Colonial Office in London was to "civilize," Christianize, and protect the indigenous people. How could this objective be accomplished in the outback where aborigines outnumbered settlers and where the white philosophy was that the only good aborigine was a dead one? Reece organizes his answers thematically rather than chronologically. He describes how the sheep station hands ridiculed and bullied aborigines, killed their women and children, and then claimed additional wages or rations for warding off "treacherous at-

tacks" by enraged male aborigines. He treats exhaustively the Myall Creek massacre, placing it in the context of racial conflict, and he notes the roles of Governor Sir George Gipps and Attorney General J. H. Plunkett in remanding the prisoners so that a second trial (which found seven guilty) could occur. There is a long, three-page quotation of Judge W. W. Burton's sentencing of the guilty parties. Reece's footnotes are extremely useful, as is his bibliography. He has made excellent sound piece of research.

Physical anthropologist David Davies' book is quite another matter. He deals with a sad and dreadful topic—the extermination of the Tasmanian aborigines. It is unfortunate that it is not well researched; there are no footnotes (though excessive use of very long quotations), and the bibliography is inadequate. Neither organization nor treatment is nearly as good as Clive Turnbull's *Black War* (1948) or Robert Travis' *The Tasmanians* (1968), and Davies' chapter on the origins of the Tasmanians (ch. 14) does not compare with the brilliance of Geoffrey Blainey's third chapter in his latest book, *Triumph of the Nomads* (1975). Davies' theme is the utter destruction of the Tasmanian aborigines within seventy-five years. In 1802 there were an estimated 20,000 of them. Then the convicts declared war on them. By 1817 there were only 7,000 left; by 1825 only 320. At this point the bricklayer-turned-evangelical protector of the aborigines, George Augustus Robinson, learned their language, lived among them, and persuaded about 195 to move to Flinders Island forty miles north of Tasmania. There he tried to Europeanize them, and there they languished and began to die. He left in 1839 to become Port Phillip protector of the aborigines. By 1848 only 40 were alive on Flinders Island, and they were returned to the mainland to a mission outside Hobart. By 1854 there were only 16; by 1869 the famous Truganina was the only one left, and she died in 1876. The illustrations, photographs, and maps are superb. They brighten an otherwise disappointing book.

SAMUEL CLYDE MCCULLOCH
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UNITED STATES

HERBERT G. GUTMAN. *Slavery and the Numbers Game: A Critique of Time on the Cross*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. 183. \$7.95.

No scholarly work published in this century has agitated the intellectual community as much as

Time on the Cross by Robert Fogel and Stanley Engerman. Heralded long before it was published, and reviewed extensively in print and on television immediately thereafter, *Time on the Cross* implicitly and explicitly attempted to correct "the traditional characterization of the slave economy," revolutionize the study of Southern slave society, and, by its novel methodology, reverse or question every assumption previously held about slavery and the Afro-American in the United States of America. That was an enormous claim not only for the work, but also for the methodology.

The deliberately intimidating format of *Time on the Cross* succeeded in blunting some early skepticism, and the work was duly hailed as a landmark, especially in the popular press. While economists attacked the validity of the model and challenged its assumptions, historians remained rather mild in their observations. Within two years, however, the situation has completely changed, and *Time on the Cross* is coming under attack from all directions. *Slavery and the Numbers Game*, previously published as a review essay, constitutes the most massive attack yet on the assumptions, methods, and implications of *Time on the Cross*. In this book Herbert Gutman has destroyed the mathematical mystique of *Time on the Cross*, punctured its claims of novelty, accuracy, and understanding, examined the past reviews of the work, and begun a new tradition in his critique.

Sometimes with humor, but more often with unmitigated venom, Gutman has accused Fogel and Engerman of egregious errors, fabrication of facts, dubious quantitative data, exaggerated rhetoric, flawed assumptions, faulty inferences, inept research, incompetent reading, confused thinking, vague generalizations, pointless questions, curious answers, unfair treatment of U. B. Phillips, Kenneth Stampp, Stanley Elkins, and others, absurd explanations, and ahistoricity. These terrible indictments, frequently repeated throughout the text, end with the painful *coup de pied* that *Time on the Cross* ends up being "old fashioned" (p. 169).

Aware that these are serious charges, Gutman goes to great pains to substantiate them, not only by reanalyzing the data presented by Fogel and Engerman, but also by introducing new data as well. The purpose is not to quibble about figures, acceptable statistics, or useful sources. *Slavery and the Numbers Game* insists that variations in percentages are irrelevant, that Fogel and Engerman cannot correct their model, and that they ought to discard it: "If the estimates were more accurate and the quantitative data examined more soundly the model meant to explain slave beliefs and behavior would still be inadequate" (p. 169). The central refutation, therefore, is that *Time on the Cross* lacks a historical perspective, that it is ahistor-

ical and cannot yield the interpretations suggested by its authors. Aggregate data, however carefully selected, cannot by themselves explain either individual behavior or individual motivation. Gutman points out repeatedly that the still-camera photographic techniques of Fogel and Engerman negate the inherent dynamism of Southern slave society. Statistics randomly selected by time and place fail to portray the pattern of changes through time and across the diverse socioeconomies of the South. This is particularly true for the Afro-American slave family, the forte of critic Gutman, who devotes nearly one-half of his book to a demonstration of the unacceptability of the conclusions of *Time on the Cross*.

In general, Gutman's arguments are persuasive. He occasionally indulges in overkill, as though it were insufficient merely to breach the dam, the stream having to be diverted above the shattered wall. Two obvious instances are his discussions dealing with occupational structure (pp. 61-78) and the age data on Southern Afro-American mothers (pp. 148-50). The essential argument in both cases is that probate records alone provide an inadequate basis for establishing either actual ages of slaves or their occupations. The analysis is persuasive, without having to resort to the equally inadequate Kentucky Union Army Rolls. Without further qualification, the geographic and economic relationship of Kentucky to Southern plantation agriculture may be no more appropriate than any Southern city as a representative sample of the occupational profile of the enslaved Afro-American population. (This reservation, incidentally, does not hold true for Gutman's use of the military census of former slaves living in Princess Anne County, Virginia, in 1866, which he employed in the slave family discussion on page 105.) Nevertheless, readers of *Time on the Cross* will find *Slavery and the Numbers Game* an indispensable companion volume and guide to the proper study of the South, econometric or otherwise.

FRANKLIN W. KNIGHT
Johns Hopkins University

EDWIN R. LEWINSON. *Black Politics in New York City*. New York: Twayne Publishers. 1974. Pp. 232. \$10.95.

In this brief volume Edwin Lewinson surveys the political status of blacks and the black political leadership of New York City from pre-Revolutionary days to 1970. The first half of the book provides a narrative of blacks' attempts to gain political office, patronage, and civil service positions. In the second half of the work, Lewinson devotes chapters to Adam Clayton Powell, J. Raymond Jones, West Indian immigrants, and the civil service.

Lewinson has undertaken a large task given the limited length of this book. He leaves, as a result, little space for analyzing the significance of the political developments he describes. He discusses economic and residential patterns in New York City, but unlike many students of Afro-American history, such as August Meier in his pioneering study *Negro Thought in America*, Lewinson fails to explore the interrelationship of political developments, social structure, and ideology. The chapters on Powell and Jones illustrate this shortcoming. They are mainly narrations without much analysis of the two men's political careers.

Since the primary focus of the volume is upon black political leaders, little room is left for examining the voting patterns of the black electorate. Lewinson frequently misses the opportunity to compare the New York experience with that of other cities and does not fully use the studies of black politics in other cities by Allan Spear, Harold Gosnell, James Q. Wilson, and Martin Kilson.

This study provides the reader with an overview of black political leaders of New York City. It would have benefited, however, from a more analytical approach.

SETH M. SCHEINER
Rutgers University

HERBERT C. KRAFT, editor. *A Delaware Indian Symposium*. (Anthropological Series, number 4.) Harrisburg: The Pennsylvania Historical and Museum Commission. 1974. Pp. vii, 160. Cloth \$4.50, paper \$3.00.

We need many more such studies as these essays on the Delaware Indians. For a hundred years teachers of American history have paid scant heed to native Americans. We have fashioned them after Hiawatha and Nokomis, set them by the Shining Big Sea Water in the ceremonials of the Camp Fire Girls, and watched their star-crossed lovers leaping off jutting river cliffs. We have filled our forests and plains with howling savages dashing out the brains of helpless infants, scalping fathers, carrying mothers to ignominious captivity. We have ignored the fact that it was they who first hybridized maize and believed that their medicine men and women were charlatans, sorcerers, magicians.

There is an abundance of material on these fictitious Amerindians. There is little reliable material available on the Indians as they actually were, especially those Indians living east of the Mississippi in colonial and frontier days before the advent of modern anthropology. There is little information on Indian cultures just prior to contact with European settlers. Though we have a few biographies of Indians such as Pontiac, we

scarcely recognize the names of other Indian leaders.

A Delaware Indian Symposium includes papers on the Indian prehistory of New Jersey; European trade relations in the seventeenth and eighteenth centuries; the Delaware language; and Delaware social organization. There is a discussion of the reasons the Delawares went to war against Pennsylvania in 1755, and also a biography of Moses Tatamy, a Delaware diplomat, landowner, and interpreter who played an important role in Pennsylvania Indian affairs during the French and Indian War. The appendix contains a short discussion of the identity of the Unalachtigo and another on Dutch loan words in the Delaware language. Excellent bibliographies accompany each paper.

N. M. BELTING
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ADOLPH L. DIAL and DAVID K. ELIADES. *The Only Land I Know: A History of the Lumbee Indians*. San Francisco: The Indian Historian Press. 1975. Pp. xvi, 188. \$9.75.

CHARLES M. HUDSON, editor. *Four Centuries of Southern Indians*. Athens: University of Georgia Press. 1975. Pp. 177. Cloth \$7.50, paper \$3.00.

Past decades have witnessed the call for greater cooperation between historians and anthropologists in studying the American Indian. More recently Indian scholars have been encouraged to contribute more to the writing of their own history and to help provide the Indian point of view that has so often eluded the best-intentioned white historians and anthropologists. The books under review show progress in these two areas.

The Only Land I Know is a brief but informative study of the Lumbee Indians in North Carolina by Adolph L. Dial, himself a Lumbee, and David K. Eliades, a Greek-American. Both are now faculty members at Pembroke State University, which was a school exclusively for Indians until 1953. The authors argue without convincing evidence for a direct connection between the Lumbee and the Croatan Indians of Raleigh's Lost Colony and then review the full span of Lumbee history with particular attention to the Lowrie Band following the Civil War, a subject examined in greater detail in W. M. Evans' *To Die Game* (1971). More recent political, social, and cultural history includes the Lumbee's challenge to the Klan in 1958 and the controversy over reaction to the militancy of the American Indian Movement (AIM). Based in part upon a large number of oral interviews, this study includes a bibliography but no documentary footnotes or index.

Four Centuries of Southern Indians, edited by the anthropologist Charles M. Hudson, contains papers presented in 1971 at the annual meeting of the American Society for Ethnohistory in Athens, Georgia. Anthropologists wrote the introduction and four essays; five more essays are by historians. James W. Covington, a historian, highlights the difficulties in relationships between the Timucuan Indians and the Spanish and French in the sixteenth century. Douglas W. Boyce, an anthropologist, argues that the Tuscaroras in the early eighteenth century included independent villages as major political units rather than a confederacy suggested by earlier writers. For the American Revolution, James H. O'Donnell, a historian, analyzes the role of major Indian groups in the South, a subject examined in more detail in his recent book. Jack D. L. Holmes, a historian, identifies the major facets of the Indian policy of Spanish governors and agents designed to thwart United States expansion during the 1790s. Re-examining the critical period of Indian removal, Arthur H. DeRosier, a historian, attacks myths found in the Choctaw experience and calls for a more accurate evaluation of the varied conduct of individual officials. John H. Peterson, an anthropologist, focuses on enclaves of Louisiana Choctaws and supplements earlier studies by identifying their success in finding an economic niche for themselves and thereby preserving much of their Indian culture. Building upon earlier studies, Raymond D. Fogelson, an anthropologist, seeks to put in proper perspective the subject of Cherokee sorcery and witchcraft by combining documentary sources with data from informants to give what he terms a "qualitative and inferential" treatment rather than one that is "quantitative and firmly documented." Albert L. Wahrhaftig, an anthropologist, probes social change among today's Oklahoma Cherokees and argues with limited evidence that the major institutional innovations come from the "full-bloods" rather than the "mixed bloods." The final selection assays the broadest theme by noting the national pervasion of racism as related to Indians and Afro-Americans.

This volume makes a valuable contribution to the study of Indians in the South even though the subjects are diverse and brevity militates against full development of some themes. The cooperation of anthropologists and historians is commendable, but continued efforts are needed by scholars in both fields to yield studies that reflect the strengths of both disciplines.

W. STITT ROBINSON
University of Kansas

DMYTRO M. SHTOHRYN *et al.*, editors. *Ukrainians in North America: A Biographical Directory of Noteworthy*

Men and Women of Ukrainian Origin in the United States and Canada. Champaign, Ill.: Association for the Advancement of Ukrainian Studies, 1975. Pp. xxiv, 424. \$27.50.

This first Ukrainian *Who's Who*, edited under the supervision of the head of Slavic cataloging at the University of Illinois Library, represents an ambitious and meritorious effort. In compiling the some two thousand biographies of notable Ukrainians, the editors employed one or more of the following criteria: current positions, duties, and responsibilities; scientific, scholarly, and professional endeavors; cultural, civic, and political activities; and former positions, achievements, and services. The entries are impeccably arranged, following the model of *Who's Who in America*, *The Canadian Who's Who*, and other such references. Most of the biographical data were obtained from the completed questionnaires furnished by the biographees, but the editors secured additional information from other sources.

The directory includes numerous biographies of former members of parliaments and governments of the Ukraine, veterans of the Ukrainian armed forces, scientists, scholars, educators, writers, and artists. One also finds in it a wide range of current activities and achievements among Ukrainians. For example, this book contains the biographies of thirty-five members of the Canadian Parliament, one Canadian senator (P. Yuzyk), one former Canadian minister of labor (M. Starr), two mayors (Winnipeg and Edmonton), one provincial governor (Saskatchewan), and several state legislators; film stars (Jack Palance and Mike Mazurki) and celebrities (Michelle Mentrinko, Miss USA for 1964, now solicitor at the Department of the Interior); seven members of the National Academy of Sciences; G. Kistiakovsky, special assistant to President Eisenhower on nuclear power; the internationally famous biologist T. Dobzhansky (who holds eighteen honorary degrees); J. Charyk, the president of Communications Satellite Corporation and the chairman of COMSAT; L. Dmochowski, the leading international authority on cancer research; W. R. Tkach, the physician to former President Nixon; W. M. Lukash, the personal physician to President Ford; T. J. Danusiari, chaplain to President Kennedy; and the champion of the housewives everywhere, Bohdan Hurko, who invented the self-cleaning oven. (I am still attempting to verify the identity of the Ukrainian who invented the pop-top can.)

Aside from the usual minor typographical errors, the most apparent shortcoming of this directory is its incompleteness. It fails to list, for example, some important members of the academic community and the various officials and staff members of the Ukrainian section of Radio Amer-

ica. The fault, of course, may not be that of the editors. As a work of reference, however, this book is invaluable. It also represents a significant cultural and sociological source, for it provides a comprehensive picture of the general tenor, direction, and concern of the sizable Ukrainian community in North America.

ALEXANDER SYDORENKO
Arkansas State University

SILVIO A. BEDINI. *Thinkers and Tinkers: Early American Men of Science.* New York: Charles Scribner's Sons. 1975. Pp. xix, 520. \$17.50.

Traditionally, the history of science and technology is written from the top—a procession of great men, events, and trends. But the middle and bottom levels are gaining attention. Perhaps the best example is Musson and Robinson's *Science and Technology in the Industrial Revolution* (1969) with its uncovering of a wide, popular diffusion of mathematical and chemical knowledge in Lancashire and the Midlands in the early years of the Industrial Revolution. Silvio Bedini wants to demonstrate something comparable for the United States from the earliest British settlements to the Jacksonian period. His model, however, is the late E. G. R. Taylor's work on the British mathematical practitioners, individuals using (or making) instruments of precision or applying mathematics, not only in science and engineering (speaking anachronistically) but also in a variety of economic activities.

Three principal theses are advanced: first, an independent movement of such individuals appeared and flourished in the America of Bedini's study; second, the movement had considerable impact in its day; third, the movement had major consequences for later American developments. Unfortunately, problems of definition and use of evidence render the work unconvincing despite a considerable array of sources. Bedini's subject is the men of "practical science," the instrument makers, mappers, surveyors, and navigators; also included are the teachers of science and "philomaths." Pure natural philosophers are excluded (were there any then?), as well as practitioners of "earth sciences," a weird term used by Bedini for natural history. (The study of geology was rare in America before 1800.) The language used is so vague that it conveys the impression that any user of a compass on land or sea is included. Carrying this form of reasoning to our day, one could describe any person switching on an electric light as a "practical physicist."

The text discloses an almost continuous influx from Britain of individuals, instruments, and literature; it even has a glowing tribute to this contri-

bution of the mother country to what hardly seems an independent movement. Disregarding these facts, Bedini stresses distinctive native practices and a lesser American professionalization. For example, British surveying practices were presumably not suitable to the American forest. Bedini then notes with triumph the appearance of the native work of John Love, *Geodesia . . .*, in 1688. But by 1792, eleven editions of the book appeared in England, indicating to me that the contents were not uniquely American.

The author apparently believes that the British (and Continental) counterparts of his subjects were largely professionally educated and employed. While a few were, Bedini is clearly not considering them in the framework of the seventeenth and eighteenth centuries. He minimizes and disregards the possibility of formal and informal apprenticeships in the United States, as in the case of youngsters learning navigation at sea from older hands. As to formal education, Bedini decries the lack of it despite his own extensive treatment of an active group of teachers largely outside the colonial colleges. The pattern of education in the United States, around 1800, is a colonial variant of that disclosed by Nicholas Hans' book on England, a work not cited here.

With his amorphous definition, Bedini cannot successfully measure the impact of the mathematical practitioners. It becomes exceedingly hard to pose and to answer meaningful questions—intellectual, social, and economic. For example, nowhere in dealing with the instrument makers does the author adequately convey the feel of life in the shop, the social role of colonial craftsmen, or the economic impact of the trade. Instead there is a self-defeating attempt to cram a diversity of types into a "little man of science" mold. A few were scientists and engineers, in our terms. Most were skilled artisans, surveyors, sailors, and so forth, and they merit treatment as such. There is nothing shameful about not being a scientist or an engineer. The approach of this book underestimates the extent to which past and recent knowledge can permeate sectors of a society, becoming part of a vernacular culture with a notable life of its own.

NATHAN REINGOLD
Smithsonian Institution

BRUCE M. STAVE, editor. *Socialism and the Cities*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. ix, 212. \$13.50.

Much of the historical investigation of American socialism has focused on national leaders and national organizations. These essays by seven scholars broaden the inquiry to the rank and file and to municipal socialism. Four chapters explore move-

ments in Milwaukee, Schenectady, Reading, Pennsylvania, and Bridgeport, Connecticut; two others examine failures in Oklahoma City and Passaic, New Jersey. James R. Green's collective biography of the most effective salesmen of the appeal to reason provides a glimpse of ordinary working socialists; much of the chapter on Reading and part of that on Bridgeport have a similar emphasis. Bruce M. Stave, the author of the piece on Bridgeport, has also contributed an introduction that relates the essays to one another and to other scholarship. A letter of Walter Lippmann, written in 1913, is printed as an appendix and argues that doctrinal failure often accompanied electoral success.

The socialist that emerges in these pages looks quite like other Americans. Salesmen of the appeal to reason shared the rural and small-town experience of their customers and, like urban socialists, came of old immigrant stock. Socialists were literate working people who enjoyed the fellowship of picnics as well as politics; new immigrants, blacks, and women were not usually admitted to the club.

Socialists won or lost elections as a result of local circumstance. They failed in Passaic because they could not enlist Slavic immigrants and succeeded in Milwaukee because of a firm base in the German community and an alliance with organized labor. George R. Lunn in Schenectady and Jasper McLevy in Bridgeport had personal, nonpartisan support. The corruption and inefficiency of opponents sometimes enhanced political opportunity. Socialist campaigners frequently ignored the program of the national party and became even less radical if elected.

Socialist activity at the local level, then, had only a tenuous connection with the Socialist party of America and owed little to Karl Marx. The socialists considered in these essays were mainstream Americans who were linked by their common willingness to ignore the ideological content of their common political label. That interpretation is in part a consequence of the format of this volume; several authors studying discrete events are not likely to produce a unified synthesis. Those who would in the future provide that synthesis will have to take account of these judicious studies.

HENRY F. BEDFORD
Phillips Exeter Academy

JOHN SHELTON REED. *The Enduring South: Subcultural Persistence in Mass Society*. Foreword by EDWIN M. YODER, JR. Chapel Hill: University of North Carolina Press. 1975. Pp. xxi, 135. \$4.95.

Is the South today really any different from the rest of the country? Most students of the region emphasize its "emergence" or "transformation"

during the period since World War II, its increasing resemblance to the other parts of the United States. In 1941 Wilbur J. Cash felt obliged to reply to those journalists and professors who said that the South was a mere figment of the imagination, that it existed only as a geographical division of the United States. He disagreed. The South, he said, is not quite a nation within a nation, but the next thing to it.

The author of the present book hardly goes so far as Cash. But, as the title indicates, the theme here is that of a persistent inner differentness which has survived the spectacular changes of the last quarter-century in the trappings of Southern life. This theme is drawn partly from a wide variety of conventional sources on the history, sociology, and political science of the region. But the core of the work is an impressive set of tables constructed from opinion surveys, most of them taken by the American Institute of Public Opinion and some by the National Opinion Research Center. The tables are supported by well-written, convincing textual analyses and commentaries.

By comparing the answers of sample groups of Southerners and non-Southerners to a series of questions on political, social, religious, and family attitudes, the author discovers a genuine regional state of mind that tends to ignore the usual sociological boundaries. He finds that whether urban or rural, rich or poor, educated or uneducated, Southerners are more likely than others to rely upon religion, parental authority, violence in settling disputes or upholding personal honor, localism in politics, and an outlook of regional defensiveness. This book reveals through survey techniques a Southern mentality quite similar to what the regionalists among the humanistic scholars and observers have already pictured more impressionistically. Hence the title *The Enduring South*.

Although this work does not answer once and for all the question to which it is implicitly addressed, it does provide data-processed evidence of the existence of a sense of uniqueness that, according to the perceptive foreword by Edwin M. Yoder, Jr., the native Southerner feels in his bones.

CHARLES P. ROLAND
University of Kentucky

RENÉ LAUDONNIÈRE. *Three Voyages*. Translated with an introduction and notes by CHARLES E. BENNETT. Gainesville: University Presses of Florida. 1975. Pp. xxii, 232. \$10.00.

For two centuries English, Spanish, French, and Swedes explored the coasts and rivers of Florida, founding and abandoning settlements. Yet few accounts, good or otherwise, in print today describe these activities. The University Presses of Florida

in 1964 published Charles Bennett's volume, *Laudonnière and Fort Caroline*, a documented account of the founding of that permanent European settlement on the northeast coast of Florida. For the present volume, Bennett translated Laudonnière's accounts of his three voyages to Florida and the Caribbean in the years 1562, 1564, and 1565. Appended are a shipping contract Laudonnière made in 1572 and a quite remarkable mutual gift agreement of the same year between the explorer and his wife. A third appendix presents Tom V. Wilder's useful study, "Plant Life in 16th Century Florida."

Though an introduction summarizes the history of the voyages, much fuller biographical material appeared in the earlier volume. Since little is available elsewhere concerning Laudonnière, it is unfortunate that the two volumes are not now published as one.

The accounts of the three voyages presumably were written solely as reports for Charles IX or Admiral Coligny and were not arguments in favor of further French colonization. Laudonnière writes in detail of the discovery of the gold fields in the southern Appalachians, which were the principal source of gold for minting United States coins before 1849. He describes his relations with the Spaniards, the English, and the Indians, none of whom he trusted. Of the Indians, however, he writes as much as did the anthropologist Tonti, who later described the Illinois Indians in *Des Gannes Memoir*. Indian women get far more attention than I have found in other histories, and Laudonnière makes it evident that among certain Florida tribes the women sometimes had considerable status and authority. They were also very good looking.

N. M. BELTING
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SACVAN BERCOVITCH. *The Puritan Origins of the American Self*. New Haven: Yale University Press. 1975. Pp. x, 250. \$15.00.

Sacvan Bercovitch is a student of early American literature who admires the artistic achievements of Puritans and who emphasizes the formative influence of Puritan ideology on American culture. In this book he explores the rich Puritan imagination for the source of the American preoccupation with the meaning of America. He finds its spring in the rhetoric of New England Puritanism. Bercovitch argues that "the myth of America is the creation of the New England Way" (p. 143).

The author approaches his task by "opening" Cotton Mather's biography of John Winthrop, "Nehemias Americanus," in the *Magnalia Christi Americana* as if he were a Puritan preacher analyz-

ing a scriptural passage to uncover its meaning. In the *Magnalia*, Bercovitch suggests, Cotton Mather created a distinctive concept of the representative American saint that differed from the traditions of medieval hagiography and from the conventions of filiopietism. Mather's rhetoric used Christology to identify the individual with scriptural heroes and used typology to unite the saint's life to sacred history. Thus John Winthrop as Nehemiah was both historical and allegorical, a representative of New England society and a forecast of the sweep of history. Mather used the word "American" in a novel way by making it express the conjunction of sainthood and nationality. The idea of an elect nation became central to the New England Way because Puritans justified themselves by justifying America. Compelled to contrive a rhetoric equal to their sense of mission, they "invented a colony in the image of a saint" (p. 114). Puritan writers could then establish the saintliness of their subjects by identifying them with the communal enterprise. Their contribution to the idea of America is revealed in the persistence of a remarkable rhetorical continuity from Puritans to Romantics. Writers from Jonathan Edwards to Ralph Waldo Emerson drew on a Puritan rhetoric that extolled the representative self as America and the American self as the incarnation of a universal providential plan.

Bercovitch's analysis is subtle and complex. Unfortunately his writing is sometimes discursive and his prose somewhat tangled; at one point the author calls his own argument "this tortuous dialectic" (34n., p. 211). Many historians will feel that Bercovitch forces rhetorical analysis to carry too heavy an interpretive burden. These are weaknesses in what is a brilliant and sophisticated study. A forthcoming treatment of the nineteenth century and Emerson presumably will carry forward the argument so well begun in this book.

GERALD J. GOODWIN
University of Houston

MICHAEL KAMMEN. *Colonial New York: A History*. (A History of the American Colonies.) New York: Charles Scribner's Sons. 1975. Pp. xix, 426. \$15.00.

According to the dust jacket, Michael Kammen's volume has a felicitous style and an urbane approach, a breadth of scholarship and richness of texture, and a judicious balance of fact and interpretation. For once, the publisher's blurbs are accurate.

New York's colonial development is not easy to describe, because it lacks the unity of purpose of a Massachusetts or the commitment to an economic system of a Virginia, both of which consequently

have some homogeneity. New York's hallmark, however, is its heterogeneity; the author's ability to deal with this while structuring a meaningful past for the colony is most praiseworthy.

Kammen has relied heavily upon the contributions of recent scholars who have delved into specific aspects of the colony's past, an indebtedness he acknowledges in a thirty-three-page bibliography organized by chapters (in lieu of footnotes) but which provides a first-rate compendium of older as well as more recent scholarship.

In synthesizing this material, Kammen occasionally lapses into interpretive errors, particularly in the 1664-1710 period. He seems, for example, uncertain about the common law's real meaning in New York (pp. 78, 129), uses various dates for the applicability of the Duke's Laws (pp. 83, 85), confuses the condemnation of "common Barrator" in the Duke's Laws with hostility toward lawyers (p. 130), wrongly identifies Nicholas Bayard as an attorney trained in England (p. 131), and confuses Lord Cornbury's place of imprisonment (p. 158). He also falls for the old canard that, except for Jacob Milborne and Samuel Edsall, "all of Leisler's close associates were Dutch" (p. 124), when actually his council of eight contained four Englishmen, and when six of the twelve close aides arrested with Leisler were Englishmen. Finally, the publisher should be chided for dropping one or more lines at the top of page 157.

Putting such criticisms aside, this volume offers a valuable blend of topical and chronological approaches, beginning with an extensive description of the area's Indian civilization and ending with the decision for independence. It deals extensively with economic, religious, cultural, and societal changes without losing sight of the chronological framework in which these occur. *Colonial New York* fills a long-standing need for an up-to-date, scholarly, and readable survey of a major development in brief compass.

LAWRENCE H. LEDER
Lehigh University

EMORY ELLIOTT. *Power and the Pulpit in Puritan New England*. Princeton: Princeton University Press. 1975. Pp. xi, 240. \$10.00.

In the last ten years the publication of fresh studies of seventeenth-century New England towns and families has made desirable a synthesis of what we know of the New England "mind" and what we are learning of New England society. The "new" social history is still very new and fragmentary. If it develops further, perhaps it will help evoke an original, general, and rounded view of the history of New England in the seventeenth century.

Emory Elliott has written a more modest book,

but he has attempted to use methods and data from several types of studies. In doing so he may alert the interested historian to new possibilities. He is interested in understanding the hold that the sermon had upon the imaginations of New England Puritans. The explanation of the power of the sermon seems to revolve around the relationship of several generations in New England. The founders, according to Elliott, repressed their children, prolonged their dependence, and loaded them down with a burden of shame and guilt for their reputed failure to live up to the standards of the first generation. The founders did this by their harsh child-rearing practices, by denouncing their sons in jeremiads, by withholding land and denying them the opportunity to marry, by keeping them from church membership, and by other unsavory practices.

Second- and third-generation ministers slowly came to realize how destructive these attitudes and practices of the founders were to religion and social harmony. As they came to this realization—especially in the 1680s and 1690s—they redefined the message from the pulpit and provided new hope, both invigorating and strengthening, to the younger generation. They accomplished this in the thirty years between 1660 and 1690 by changing in the sermon from “one dominant archetype—the image of the angry and wrathful God the Father—to another archetype—the figure of the gentle, loving, and protective Christ.”

Elliott devotes a little more than half the book to the examination of the transformation of the sermon. By concentrating on the form and content of the message to the rising generation he offers an interesting, albeit incomplete, explanation of how Puritan culture sustained itself in the second half of the seventeenth century. His analysis of the language of these sermons seems especially valuable. This part of his book is more convincing than the first, which lays too much weight on the still fragmentary findings of recent social historians. It is not yet clear, for example, just how widespread was the practice of fathers withholding land from sons. Elliott may also lean too heavily on Erik Erikson in reconstructing the inner lives of Puritans; his borrowing from Erikson may not reckon with the important differences between seventeenth- and twentieth-century psyches. But Elliott's use of other disciplines is done with an admirable tentativeness, and his conclusions should be received in the same way.

ROBERT MIDDLEKAUFF
University of California,
Berkeley

E. BROOKS HOLIFIELD. *The Covenant Sealed: The Development of Puritan Sacramental Theology in Old and New*

England, 1570–1720, New Haven: Yale University Press. 1974. Pp. xi, 248. \$12.50.

In the years since the publication of Perry Miller's *The New England Mind: The Seventeenth Century*, much of the scholarship on Puritan studies has taken the form of either elaboration on Miller's themes or attempts to refute them. Yet occasionally a work appears that illuminates an aspect of Puritan thought largely ignored by the Harvard scholar. *The Covenant Sealed* is such a book. In it E. Brooks Holifield carefully untangles the complex and often contradictory strands of sacramental theology advanced and debated in English and American Puritan circles from 1570 to 1720.

Holifield opens his study by setting forth the role of sacramental controversy in the fragmentation of early Protestantism and by outlining the attitudes of Luther, Zwingli, Calvin, and later reformers on issues such as the eucharistic presence and the efficacy of the Protestant sacraments of baptism and the Lord's Supper. He proceeds to explain the evolution of early Puritan views, emphasizing the pastoral concerns of the clergy and their integration of sacramental theory with covenant theology. Though their views were permeated with ambiguities and ambivalence, these clergymen believed that sacraments were seals of existing covenantal relationships rather than converting ordinances. Holifield describes the seventeenth-century debates that such ambiguities occasioned both in England and in New England. He is one of a growing number of scholars who is tuned to the importance of viewing the American Puritans in an international context, not just for the first two decades of New England settlement but for the entire colonial period. Indeed, his exposition of the growth of New England sacramental thought argues persuasively that the main lines of the debate between Stoddard and the Mathers were set by English Nonconformist divines. In his concluding chapters Holifield traces a revival of sacramental piety in the late colonial period and suggests reasons for that development.

While the prime focus of *The Covenant Sealed* is sacramental theology, the author is aware of how changes in that area were indicative of shifts in the New England mind that were manifested in other areas as well. One of the strengths of his analysis is his skill in indicating these connections. He notes the parallelism between the increasing symbolism in Puritan sacramental thought and the development of more elaborate forms of tombstone art. Similarly, he relates the increasing sacramentalism of the late seventeenth century to the movement toward a sacerdotal concept of the ministry as recently described by David Hall. Compared to the volume's strengths, its shortcomings are mi-

nor. Holifield shows how the changing sacramentalism of New England Puritans was in part influenced by alterations in their social and institutional needs and circumstances, but he fails to provide a comparable analysis for English intellectual developments. There are also a few minor errors; for example, John Goodwin was not the brother of Thomas Goodwin, nor were they related. But such caveats should not appreciably affect the book's reception, for it is a significant analysis of an important topic in the intellectual and religious life of the trans-Atlantic Puritan community.

FRANCIS J. BREMER
Thomas More College

CLARENCE L. VER STEEG. *Origins of a Southern Mosaic: Studies of Early Carolina and Georgia*. (Mercer University Lamar Memorial Lectures, number 17.) Athens, Ga.: University of Georgia Press, 1975. Pp. xiii, 152. \$6.00.

One finds upon reading this excellent monograph that the first two essays are less satisfactory than the last two chapters. This difference in quality arises primarily from the lack of reliable sources and the necessity of interpreting the kaleidoscopic events that prevailed in the early history of the two Carolinas. Out of the conflicts and confusion, however, one clear theme predominates—the determination of the settlers to assert the rights of Englishmen to self-government. Contrary to the Turner frontier thesis, the demand for the establishment of more democratic institutions in the Carolinas and Georgia had little to do with the influence of the frontier but came instead from England and from notions of the rights and privileges of Englishmen, as proclaimed especially at the close of the eighteenth century in the Declaration of Rights.

This small volume, rich in insights and fresh interpretations, attacks certain "assumptions" of early Southern history. The author maintains, for example, that the slaves were not regarded as ignorant and incompetent because they were gifted tropical farmers, were enrolled in the militia, served along with Indians to defend the colony from the Spanish, and were used as cowboys on the thriving cattle ranches. Before the plantation system arose in South Carolina, the export of deerskins and Indian slaves was the most important commercial activity. The startling development of the naval stores industry after 1705, when Parliament offered a bounty for their production, and not the growth of rice culture, the author thinks, explains the rise in slave importation in early eighteenth-century Carolina. Before the boom in naval stores, relatively few slaves were imported. The coincidence of slavery expansion

with the development of naval stores does not, however, support his thesis except as one contributing factor. I could not discern how the great diversity of nationalities in the population and the turbulent history of the colonies contributed to the rise of the concept of "the South" any more than a similar diversity of population in Pennsylvania or New York imparted "a special quality" to those states. The essay on Georgia, where the sources are more abundant than on the Carolinas, is a little gem.

CLEMENT EATON
University of Kentucky

EDMUND S. MORGAN. *American Slavery, American Freedom: The Ordeal of Colonial Virginia*. New York: W. W. Norton and Company, 1975. Pp. x, 454. \$11.95.

At the heart of this book is a scintillating account of the first six decades of the history of Virginia. The pages devoted to the period between 1600 and 1660 rest upon careful research on both sides of the Atlantic, and they make imaginative use of fresh materials in the county court records. Thoughtfully and critically written, they present a lucid, occasionally witty, analysis of the transformation of a commercial company into a settled society, related to but different from its English antecedents. This part of Edmund S. Morgan's volume will long reward its readers. The flaws in it are minor.

The chapters that precede and follow it, however, create serious problems. They present a novel, challenging, but largely erroneous, interpretation of the relationship of slavery to freedom in colonial Virginia.

The question asked is that which troubled the revolutionary generation. How was it possible to reconcile the dedication to liberty of the great Virginians who participated in the movement for independence with their acceptance of slavery at home? Morgan answers that slavery and freedom not only coexisted, but that the two conditions were locked into a symbiotic relationship in which slavery made possible the freedom Jefferson's contemporaries espoused. The evidence is not adequate to support these sweeping conclusions.

The opening chapters show that Englishmen before Jamestown were not racists; they conceived of a future society with places for both Indians and whites. The arrival of blacks scarcely affected attitudes toward color through Virginia's first half century. The development of tobacco culture and the plantation, the appearance of slavery and racism, and ultimately the emergence of a political system that united the interests of large landowners and yeomen changed those attitudes. The

republicanism toward which Virginians moved in the middle of the eighteenth century was compatible with slavery because the English Commonwealthsmen from whom they adopted it did not scruple to thrust their own poor into bondage.

This ingenious, if tortuous, argument occasionally emits a burst of illumination, but more often is wrong-headed. The inability to define such concepts as plantation, slavery, and race exposes its grave distortions.

"Virginia slaves were introduced into a system of production that was already in working order." That sentence opens a key paragraph pointing out that the substitution of slaves for servants increased the profitability of the plantation system, but required new methods of discipline linked to racial contempt (p. 316). All the terms are questionable. The plantation into which blacks were introduced did not employ gang labor and was, therefore, not the same as that which took form after the slaves appeared. The identical word, plantation, appeared in 1607 and in 1667 and in 1767, but it bore entirely different meanings at each date.

Slavery was a slippery word in the seventeenth century and grew only slightly more precise in the eighteenth when the lawyers set to work on it. The assertion that James Burgh, one of the English Commonwealthsmen, "wanted to enslave 'idle and disorderly persons'" (p. 382) is correct. But his proposal that "a set of press gangs" seize and set to work "during a certain time" all "idle and disorderly persons, who have been three times complained of before a magistrate" (p. 324), was by no means analogous to the bondage of blacks at the time, which in turn differed from the servitude of 1650.

Morgan's meaning for racism is also unclear. He presents no evidence of views similar to those of the proslavery apologists of the nineteenth century. And if he has in mind the belief that Indians and blacks were inferior, brutish peoples, then that belief also existed in the 1580s and applied as well to the Irish.

OSCAR HANDLIN
Harvard University

LESTER B. SCHERER. *Slavery and the Churches in Early America, 1619-1819*. Grand Rapids: William B. Eerdmans Publishing Company. 1975. Pp. 163. \$5.95.

For analytical purposes Lester B. Scherer divides his survey of the relationship of the early American churches to slavery into three parts: the colonial period, 1619-1700, the provincial, 1700-63, and the revolutionary-early national, 1763-1819. Within each division he devotes one chapter to the history

of slavery and then follows with one that focuses specifically on the relationship of Christianity to slavery.

Scherer finds little to distinguish Christian attitudes toward, and treatment of, slaves from those of early American society at large. For the most part, Christians thought of blacks as a people set apart, to be judged by criteria quite different from those applied to whites. He concludes that "one comes away from the materials of the period with the overwhelming impression that the churches generally related passively and permissively to the dehumanization of blacks."

Christian opposition to slavery occurred, but it was located primarily in groups alienated in one way or another from the larger white culture. Quakers consistently proved the most outspoken critics of slavery. In their formative years the Methodists prohibited slaveowning among their preachers, although the force of this prohibition weakened as their numbers grew and as the church became less sectarian in outlook.

Scherer leaves two areas largely unexplored. He says little about black Christianity, an understandable omission given the current state of the historical literature. Further, he does not deal at length with one important question—why most Christians supported slavery while only relatively few did not. The difference was seemingly rooted not only in doctrine but also in the social function of the churches. The matter merits serious attention, for it offers one way to understand better the social role of Christianity in early American society.

M. L. BRADBURY
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College Park

RICHARD P. GILDRIE. *Salem, Massachusetts, 1626-1683: A Covenant Community*. Charlottesville: University Press of Virginia. 1975. Pp. x, 187. \$8.75.

Richard P. Gildrie's *Salem* is the latest in the genre of town-based, early New England studies that has, over the past decade, altered our view of the Puritan domain. Indeed, Gildrie's basic assumptions are the very product of that genre. Diversity, not homogeneity, marked the section, yet there was a rough ordering to the towns, "a spectrum ranging from the stable, usually inland, agricultural communities where Puritan organicism remained essentially unchanged for decades to the rapidly changing port of Boston where institutions adapted swiftly to the pressures engendered by the town's commercial and political role" (p. ix). Above all, the symbolic language of New England—in this case, that embodied in Salem's covenants—is not a distinct entity to be studied apart

from social realities. It affects those realities and in turn is affected by them.

Gildrie offers nothing spectacularly new. We have seen before (in John Waters' work on Hingham) the divisions between old and new settlers and between settlers from one part of England and from another. We have seen the effects of commercial growth (in my study of Boston), the alienation of parts of a New England township from the town center (in Kenneth Lockridge's work on Dedham), the tendency toward an unequal distribution of wealth and political power (via William Davisson and others), and the elitism, deference, and occasional insurgencies within the towns (Lockridge again). This observation, however, is not a criticism. The insistence that every historian break new ground in every work is the bane of the profession; certainly it unhinges completely the case study method.

If Gildrie has neither offered a new thesis nor overturned an old one, he has in good workmanlike fashion presented the kind of evidence that is necessary for making generalizations about early American society. One might wish that he had paid more (or less) attention to this or that aspect of the town. With his command of the Salem materials, for example, Gildrie might well have contributed to the demographic base being created; there is nothing here but a shallow bow toward the demographers. One might wish, too, for a stronger, keener interdisciplinary stance. An evening with a sociologist discussing status consistency vis-à-vis status inconsistency might well have given Gildrie a new perspective from which to view Puritan organicism. Another such evening—this time discussing "community" as an abstract concept—might have led him to a better understanding of both the communal aspirations of the seventeenth century and the functioning of seventeenth-century communities. As it is, the two are regularly confused. Nevertheless, one comes back to the main point: this is a sound, workmanlike case study.

DARRETT B. RUTMAN
University of New Hampshire

RICHARD WALSH and WILLIAM LLOYD FOX, editors. *Maryland: A History, 1632-1974*. Baltimore: Maryland Historical Society. 1974. Pp. xvi, 935. \$12.50.

This narrative history serves better as an introduction to, rather than as the final statement on, our present knowledge of Maryland's past. The ten essayists view events from a national and chronological perspective, placing Maryland in the mainstream of American history; the result is an emphasis upon institutional, social, and political topics. Unlike earlier Maryland histories, over

half this volume covers the years from the Civil War to the present. Short bibliographies follow each essay.

Perhaps the most significant problem with this book is that its narrative format frequently obscures the process of change under the guise of chronological continuity. Many of the essays demonstrate the importance of Baltimore in the state's social and political history, yet no chapter is devoted to it. This is unfortunate because the city's social and economic diversity, its wealth, and its growth as a mercantile city had a statewide impact. Indeed, much of Maryland's nineteenth- and twentieth-century political and social history can just as easily be explained in terms of an urban-rural, intrastate conflict rather than as a reflection of national patterns or trends. The treatment of industrialization shows another aspect of this same flaw. While no one would question the utility of a descriptive chapter on this important topic, it is significant that its preindustrial stage needs equally careful analysis. The volume even lacks any systematic study of agriculture. Considering that regional specialization has long been a characteristic of Maryland agriculture and that farming and farm service have brought sustenance to many Maryland citizens, this omission is incomprehensible. Similarly, social groups (such as immigrants and blacks) or trends not neatly fitting into national periodization (such as race relations or urban-rural conflict) are treated episodically and amorphously.

Perhaps we can better understand these problems when we appreciate the absence of much creative work in local history. The excellent social demographic studies of colonial Maryland being done by the St. Mary's City Commission or those being conducted by nonhistorians, notably historical geographers, should be represented in such a general history. It is ironic that this state history, given its national emphasis, poorly reflects local history.

WHITMAN H. RIDGWAY
*University of Maryland,
College Park*

SANBORN C. BROWN and LEONARD M. RIESER. *Natural Philosophy at Dartmouth: From Surveyors' Chains to the Pressure of Light*. Hanover, N.H.: University Press of New England, for Dartmouth College. 1974. Pp. xii, 127. \$6.50.

At a time when American colleges and universities are facing a forced redirection, particularly in science, this modest volume presents a most enlightening account of how scientific research was conducted and supported at Dartmouth College over a period of more than a century. In a series of short

historical sketches of activities at Dartmouth in the area of natural philosophy, now known as physics, the authors trace the evolution of a science department, dealing not only with the curriculum but with the individuals involved. The work has a telling impact, for the reader is made aware of what happens to the educational process when scholarship is not only discouraged but the search for new knowledge is not supported. It becomes clear that there was relatively no communication between institutions of learning, an isolation arising from the practice of colleges to hire their own graduates. The book is illustrated with twenty-five interesting photographs of the major figures, sites, and equipment relating to the study of physics at Dartmouth.

Following his establishment of Moor's Charity School in Lebanon, Connecticut, Eleazar Wheelock founded Dartmouth College in 1769, moving it to Hanover in 1770. One of Wheelock's students at Moor's, Bezaleel Woodward, graduated from Yale and returned to work with Wheelock. He eventually became a professor at the new college at Hanover and served it in various capacities; he was treasurer, librarian, trustee, president pro tempore, and even contractor in charge of the construction of Dartmouth Hall. There were other figures as well, such as the unflappable John Smith, who became professor of languages and who during his years as tutor recorded the curriculum in natural philosophy as it existed under the college's first two presidents. John Hubbard, who succeeded Smith as professor of natural philosophy and mathematics, proved a much more forceful figure. The teaching of natural philosophy at Dartmouth, however, was a static discipline and rigid in its offerings through the tenure of Ebenezer Adams, who taught until 1833. It was under Adams that the first scientific observations were undertaken at Dartmouth, with the initiation of a "thermometrical register" maintained by his son, and which has been continued at the college to the present time.

With the addition of Ira Young to the faculty as professor of natural philosophy, the science curriculum emerged from the doldrums. Young acquired new science teaching apparatuses to supplement the few outworn instruments. Furthermore, his enthusiastic teaching and the new equipment stimulated interest in natural philosophy to the degree that when an appeal for public support of the college was made in 1841, the largest donation was given to endow a professorship in natural philosophy. With the death of Young in 1858, Dartmouth learned the lesson that one-man departments in small colleges made no allowances for continuity. His successor, Henry Fairbanks, emphasized even more Young's great

vitality. With the appointment of Ira Young's talented son, Charles, the focus of scientific study became astronomy. He succeeded in acquiring better instruments for the support of research astronomy and personally made a number of substantial contributions.

Charles Emerson, who succeeded Young, saw the advent of physics as part of the curriculum and the end of the single-man department at Dartmouth. At the end of the nineteenth century, the department of physics assumed a greater role in original scientific research under the management of Edwin Brant Frost, A. C. Crehore, and Ernest Fox Nichols, who invented the Nichols radiometer and cooperated with G. F. Hull in experiments on the pressure of light.

The authors succeed in maintaining historical continuity in describing the science program from its early pedantic teaching to vigorous research, through the change in productivity from faculty concentration on teaching to administratively supported faculty research. The work provides a vivid re-creation of the austere student and faculty life in a small New England college in the late eighteenth and early nineteenth centuries. Despite its modest size, *Natural Philosophy at Dartmouth* is a valuable record that successfully illuminates yet another dimension of early American science, which will prove of lasting value for the historian and student in this field.

SILVIO A. BEDINI
Smithsonian Institution

L. H. BUTTERFIELD *et al.*, edited and with an introduction by. *The Book of Abigail and John: Selected Letters of the Adams Family, 1762-1784*. Cambridge, Mass.: Harvard University Press. 1975. Pp. ix, 411. \$15.00.

This volume is what publishers call a coffee-table book: a handsome volume for the general reader who wishes to dip into a subject and be entertained and instructed by it without mastering its complexities. To promote a brisk sale, the Harvard University Press publicized the book vigorously and brought it out just before the Christmas season. It was also timed to coincide with the Bicentennial and with the appearance of the television series, the "Adams Family Chronicles." Some of the profits of the volume will presumably be used to subsidize the scholarly edition of the Adams Family Papers now underway at the Massachusetts Historical Society. Therefore, one wishes it well as a commercial venture.

The subtitle of the volume is somewhat misleading, for it contains not only the letters of John and Abigail Adams, but entries from John Adams' diary that illuminate the relationship between the

famous couple. The letters and diary entries are connected by a stream of editorial headnotes. There are no footnotes. Virtually all the material in the volume has appeared in L. H. Butterfield's magisterial editions of the *Diary and Autobiography of John Adams* and the *Adams Family Correspondence*. The scholar will, of course, want to use these volumes rather than the distillation under review. Yet so well selected are the letters in the present volume, so informative are the editorial notes (in which the editors, at times, throw off some of their reserve—so necessary and appropriate in the comprehensive edition of the Adams Family Papers), so charming is the correspondence, that the volume could be used with advantage in an undergraduate course as a way of introducing beginning students to collections of source materials.

As one would expect, there are no revelations in the collection. Abigail at times looms larger than John, not only because the editors choose to stress her accomplishments, but because much of John's wartime correspondence was brief and bland, a necessary precaution because of the vulnerability of letters to spies and the British navy.

John and Abigail Adams have captured the American imagination during this Bicentennial season, owing, in no small part, to the work of Butterfield and his colleagues in meticulously publishing their writings over the past fifteen years. The success this pleasant volume will enjoy is as much a tribute to them as to the Adamses themselves.

JAMES H. HUTSON
Library of Congress

DAVID AMMERMAN. *In the Common Cause: American Response to the Coercive Acts of 1774*. Charlottesville: University Press of Virginia. 1974. Pp. xii, 170. \$9.75.

CAROL BERKIN. *Jonathan Sewall: Odyssey of an American Loyalist*. New York: Columbia University Press. 1974. Pp. xi, 200. \$10.95.

JOHN M. COLEMAN. *Thomas McKean: Forgotten Leader of the Revolution*. Rockaway, N.J.: American Faculty Press. 1975. Pp. xv, 332. \$16.95.

The three books reviewed here are diverse in interpretation, topic, and chronology, but they have a unifying theme: the reaction of Americans to British policy before, during, and after the American Revolution. Carol Berkin's biography of Jonathan Sewall relates the story of a Massachusetts loyalist; John M. Coleman's book is a history of Thomas McKean, a patriot from Delaware-Pennsylvania; and David Ammerman's volume analyzes American response to the Coercive Acts in the years 1774 and 1775.

As a doctoral dissertation Berkin's work on Se-

wall received the Bancroft Award from Columbia University. The prize was deserved, for the author tells Sewall's story well and briefly. If her book has a fault, it is that her narrative at times is almost too truncated; for instance, she could have said more about Sewall's private life. It would appear, however, that Berkin included all pertinent information that her sources yielded—if at times in brief snatches and in footnotes.

Her description of the origins of Sewall's loyalty in his rise from childhood poverty with the assistance of well-placed conservative men is convincing. The author's deftness in handling Sewall's political writing is commendable. The psychological portrait of Sewall as a man who sought to retreat from trouble rather than face it is both poignant and supported by her evidence. Her analysis of Sewall in the years after he went into political exile and lost his prestige and power in Massachusetts society is particularly well written and touching.

Unlike Sewall, the Thomas McKean of Coleman's portrait was rewarded for devotion to his cause. McKean was an important patriot-politician in the middle colonies during and after the Revolution, and Coleman's listing of the positions McKean held attests to the author's argument that he deserves biographical treatment. In some ways, however, Coleman's biography does not fill the need. While generally well written, the book contains some curious stylistic slips that force the reader on occasion to reread sentences and phrases for meaning. The author asserts that this work is a biography, yet in many places he wanders from his subject and discourses excessively on peripheral topics that have little bearing on McKean's life. The book is advertised as being the complete story of McKean, but it stops abruptly in 1780, thirty-seven years before the man died. As a consequence, the reader is left with little feeling that he has come into contact with a real historical figure.

Interestingly enough—since Coleman says he intended to write a biography—the greatest strength of his book (aside from its copious research) is its analysis of Pennsylvania politics in the period leading to the Declaration of Independence. The author deftly shows how divided the colony was on the question of independence and how "radicals" like McKean were forced to manipulate politics to assure "unanimity" on this important matter. It seems clear from Coleman's writing that even as late as June 1776 there was no consensus among Americans on independence. A powerful conservative element at that time wanted to await events in the fond hope that a reconciliation could take place between Britain and its American subjects.

Coleman's point seems to fly in the face of David Ammerman's thesis in his book on the Coercive Acts. Ammerman contends that American resistance to these acts, more than a year before the Declaration of Independence, marked the turning point of the colonies toward revolution. Ammerman says that the striking thing about the American response to British coercion is that a consensus was achieved which cut across political and class lines, reaching a high-water mark in the First Continental Congress, where both conservatives and radicals united in opposition to a number of laws passed by Parliament.

It appears that Ammerman has overstated his thesis, for one need only look at Coleman's survey of Pennsylvania politics to see that American dissension on how to respond to Britain's coercion was not stifled in 1775. Ammerman only manages to show that Britain's attitude toward America had hardened and that it was in London where consensus was achieved on forcing the Americans to yield to parliamentary authority.

The real strength of Ammerman's compactly written book lies in his excellent survey of how the patriots organized the political structures of resistance, first the committees of correspondence and later the powerful committees of inspection set up to enforce nonimportation, nonexportation, and nonconsumption agreements. Such committees were used by patriots, at least throughout 1776, to suppress continuing opposition to their policies from conservative men like Sewall, who were not included in the consensus of 1774.

In looking at these three books, it seems obvious that Americans responded to the Revolution in various ways. It is also clear that the quality of scholarship in Revolutionary history is diverse. Berkin's and Ammerman's books are filled with penetrating insights and are soundly organized. Coleman's biography of McKean also contains some cogent observations, but the quality of the study is weakened by unnecessary and unfortunate digressions that overshadow the subject of the book.

PAUL DAVID NELSON
Berea College

CHARLES S. OLTON. *Artisans for Independence: Philadelphia Mechanics and the American Revolution*. Syracuse, N.Y.: Syracuse University Press. 1975. Pp. xii, 172. \$9.95.

This book is a welcome addition to our historical literature. Philadelphia, the second largest city in the British empire, commanded a leading economic position in the Middle Colonies, and knowledge of its politics is vital to understanding the Revolutionary movement.

The Philadelphia mechanics, colonial America's skilled "blue collar" workers, held a place of high esteem in the city's largely commercial, bourgeois social structure. They became politically galvanized after the Stamp Act and as a political block played an increasingly larger role in resisting British taxation and regulation. The mechanics' interest was heightened by boycotts of British goods which they helped enforce and from which they benefited. By 1776 they were among the chief advocates of independence. After independence they initially supported the radical Constitutional party for the political freedoms it offered; later they backed the Republican party for the economic stability it promised. The mechanics supported the federal constitution because it allowed the new government to inaugurate national economic programs. On this point the Philadelphia counterparts were economic nationalists.

This otherwise good work on the mechanics is marred, because the author raises questions that are unanswered. At times there are puzzling contradictions. He claims that the mechanics were not proletarians, but he does not examine probate, tax, or similar records to establish the point. He says the mechanics favored revolution because they were manufacturers in competition with England's manufacturers. While many artisans were manufacturers, not all were. Why were nonmanufacturers on the side of Revolution? The author scores economic determinism, perhaps justifiably in the case of the mechanics, yet the expressions he uses, together with the quotations from the mechanics, definitely have an economic bias. He says the mechanics were not radical, and yet the reader finds that they often favored radical measures, and often supported Philadelphia's poor, of which there were a growing number. Why? Too many puzzles still remain about this important Philadelphia and national group.

RICHARD WALSH
Georgetown University

ROBERT A. EAST and JACOB JUDD, editors. *The Loyalist Americans: A Focus on Greater New York*. Tarrytown, N.Y.: Sleepy Hollow Restorations. 1975. Pp. xiv, 173. \$12.00.

Here is yet another example of the revived scholarly interest in the loyalists that has accompanied the Bicentennial celebration. The editors of this volume have collected a group of papers originally presented at a conference in Tarrytown, New York, during the fall of 1973. In addition, they have included ten handsome illustrations, a rather cursory bibliography, and the complete documentation supporting the claim of the Philipse family to

the Royal Commission of Enquiry. The essays themselves are of uneven quality and significance.

In a tantalizingly brief opening essay, John Shy discusses the significance of the British military use of the loyalists in the Hudson Valley and suggests a new and fascinating perspective on the relatively pacific consequences of America's Revolutionary experience. But succeeding essays by Catherine Crary and Jacob Judd are weakened by a tendency to become mired in a morass of insecure detail. Furthermore, Crary fails to provide a persuasive argument for the significance of "De Lancey's Cowboys," while Judd's explanation of Frederick Philipse's loyalism is disappointingly superficial. Similarly, William Benton's essay on Peter Van Schaack does little more than repeat an interpretation the author has articulated more fully elsewhere, and, in any case, Benton's treatment pales in comparison to Carl Becker's elegant essay ("John Jay and Peter Van Schaack") written in 1919.

Despite these weaknesses at the book's center, however, the final two contributions are excellent. Willard Randall's analysis of William Franklin is thoughtful and admirably sensitive to the ambiguities in the New Jersey governor's attitudes and character, and the article also contains some provocative insights into William's relationship to his more famous father, Benjamin. The last piece in the book, Esmond Wright's interpretation of the New York loyalists as a cross section of colonial society, provides an appropriate conclusion, embodying perceptive suggestions both about loyalism and the complex character of New York society and politics in the Revolutionary era.

Like most books of its genre, this collection whets rather than satisfies the appetite of scholars, yet it contains some fine original research and, for that reason, deserves careful consideration by historians concerned with the losers in America's first civil war: the loyalists.

DOUGLAS GREENBERG
Lawrence University

DONALD J. D'ELIA. *Benjamin Rush: Philosopher of the American Revolution*. (Transactions of the American Philosophical Society, New Series, volume 64, part 5.) Philadelphia: the Society. 1974. Pp. 113. \$5.00.

Benjamin Rush has a unique position among the revolutionaries of 1776; he was a reformer who saw the Revolution as an opportunity to transform America into a Christian republic. He condemned slavery, helped form the first abolitionist society in America, and donated land in western Pennsylvania so that former slaves could become free-soil farmers. He believed in a liberal education for everyone, and he specifically included women in

his plans because their enlightened influence on society could help make the republic a better place to live. Rush also challenged traditional education, raised questions about intemperance toward liquor, and urged a rededication to Christian principles. He was a deeply religious man, not tied to any denomination, who believed in equality of men, human perfection, and an eternal life.

With such a bent toward reform, Rush should be popular with modern historians and progressive Americans, but he remains obscure and unappreciated. D'Elia's study makes a solid contribution by putting Rush's bright ideas into the intellectual culture of early America, but it fails to bring excitement, clarity, and drama to those ideas and lacks a comprehensive view of Rush's theories. His medical ideas and practices disappear into the background, while his religious, moral, and ethical ones seem to have too much importance. His interests in politics and the new nation also give way to religion or ethics. In the narrative, other philosophers obscure Rush so that his general position as a second-rate hero of the Revolution seems reconfirmed. Intellectual history of this kind, however, is difficult to write; balances, relationships, and explanations require skill as well as insight. This fine study has much in content and analysis that advances knowledge of Rush.

JOHN A. SCHUTZ
University of Southern California

EMORY G. EVANS. *Thomas Nelson of Yorktown: Revolutionary Virginian*. (Williamsburg in America Series, 10.) Williamsburg, Va.: The Colonial Williamsburg Foundation; distrib. by University Press of Virginia, Charlottesville. 1975. Pp. x, 204. \$8.95.

Thomas Nelson is little known, even though he was a signer of the Declaration of Independence and the governor of Virginia during the trying months of the British invasion of that state in 1781. The guides at the Yorktown battlefield point to his house in the town and speak of the selfless patriotism of Nelson, who ordered that artillery fire be aimed at his own mansion, supposing it to be the headquarters of Lord Cornwallis. But in spite of these claims to honor, he is generally assumed to be a rather undistinguished member of the eighteenth-century Virginia gentry.

This valuable biography by Emory G. Evans of Northern Illinois University corrects this mistaken impression and saves Nelson from relative obscurity. It has not been an easy task: Nelson is difficult to understand, and there are apparent inconsistencies in his character and career that have to be resolved.

The Nelsons were not a typical family of the Virginia elite. Unlike the Byrds, the Corbins, the

Randolphs, and the Lees, whose wealth and prominence in the affairs of the colony dated back to the seventeenth century, the Nelsons were newcomers. Thomas' grandfather arrived in Virginia in 1705, prospered, and pulled himself up from middle-class status to the gentry. In contrast to the older aristocracy, the family derived its wealth from trade, not land. But by 1730 the Nelsons were clearly among the hundred or so ruling families.

Thomas Nelson, Jr. is portrayed in this thorough, painstaking biography as a man of considerable ability, especially in public finance, and clearly one of the important shapers of the fortunes of the state. He emerges as a credible human being, occasionally dissipated, inclined to obesity, plagued by poor health, yet enormously energetic and resourceful. In addition to skillfully managing the task of supplying money for the War for Independence, he contributed much of his own money and personal credit.

WILLIAM M. DABNEY
University of New Mexico

RICHARD H. KOHN. *Eagle and Sword: The Federalists and the Creation of the Military Establishment in America, 1783-1802*. New York: The Free Press. 1975. Pp. xx, 443. \$13.95.

Richard Kohn's work focuses on the military, yet it includes social, intellectual, and political aspects of the post-Revolutionary period. He believes that two conceptions of American defense emerged and the structure of the army became "the key issue" (p. 296) of the 1790s. The nationalists advocated centralization, professionalization, and preparation. Their opponents supported a decentralized militia system. These attitudes reflected divergent world views.

To the nationalists a standing army was a dependable institution in a disintegrating world, a symbol of order and stability. Distrustful of democracy and afraid of mob violence, they believed that the real danger lay not in Europe but in American dissent. Events convinced them that Republicans were part of widespread efforts to undermine the American experiment and civilization itself. The New England apocryphal psychology and jeremiad tradition sensitized them to feelings of doom. History was cyclical; every sign of weakness indicated decay. No tradition of loyal opposition existed. They believed that their generation was exceptional, so every event took on extra significance. This all added up to a cast of mind that "drove them toward a pessimistic and malevolent interpretation both of events and of the character of the opposition party" (p. 217). The new army of 1798, conceived as a "bulwark against rebellion" (p. 239), would insure stability. Officers were cho-

sen for their ideological reliability. When the nationalists divided over the question of defense, "the first and only completely political army in American history" was dismantled (p. 267).

Kohn is revisionist yet not polemical. Deep divisions existed in the early republic, and there were continuities between the factions of the 1780s and the parties of the following decade. The military establishment was not primarily a response to need, but was political "in the purest sense of that term" (p. xii). On several occasions the nationalists attempted to use the army to influence the development of government. They had a militaristic streak that appeared only in times of crisis.

Eagle and Sword is an example of the finest in American historical literature. Detailed and often subtle, it is far more complex than this brief essay can indicate. Kohn grounds informed speculation on ten years of exhaustive primary and secondary research. He is aware of Kurtz, Buel, Smith, and others who have written on the period and analyzes his relationship to them. More important, *Eagle and Sword* carefully treats an important and neglected topic in a fashion that illuminates the period as a whole.

RICHARD M. ROLLINS
Ohio State University

DANIEL SISSON. *The American Revolution of 1800*. With an introduction by HARVEY WHEELER. New York: Alfred A. Knopf. 1974. Pp. xvii, 468, vi. \$12.50.

Daniel Sisson charges that in following Henry Adams' interpretation of the election of 1800 as a "pseudo-revolution," historians have "revealed that they failed to comprehend the nature of revolution." Properly understood—and to Sisson this "requires a mind that is somehow attuned to revolutionary times"—that election did not establish "a new mode of political organization, viz., [sic] that of political parties," but a model for revolution. Sisson maintains that "it is this book's contention that when Jefferson wrote, 'The Revolution of 1800 was as real a revolution in the principles of our government as that of 1776 was in its form,' he meant precisely that." To the author, the election also represented "a repetition . . . of the early phases of the French Revolution of 1789."

On what grounds does Sisson accept as literally true an assessment by Jefferson that most other historians view as a pardonable instance of partisan hyperbole? On what basis can he plausibly argue that one of the closest elections in our history, a contest that resulted primarily in a change of men and not of measures, constituted a revolution? He handles such questions by centering on the antecedents of the "revolution" and not its fruits.

Asserting that "those who are revolutionary 'quick-witted'" can detect his "indebtedness to [Bernard] Bailyn's masterful work" on the Revolutionary era, the author announces that he intends "to establish an ideological framework for revolution as it developed during the decade after the Constitution." That ideological framework, it turns out, is the familiar Jefferson-Hamilton dispute. Despite Sisson's disclaimer that he sought "to avoid the black-and-white perspective of those historians who have set Jefferson against Hamilton as two cocks in a pit," he unhesitatingly jumps into the political arena of the 1790s, presenting an assault on Hamilton and a defense of Jefferson and his allies at which all but the more ardent Democratic-Republicans of that period might have blinked. Maintaining that Jefferson "acted to preserve the spirit and principles of the American Revolution," Sisson proceeds to the highly disputable contention that these principles were threatened by the proponents of monarchy led by Hamilton, a New World "Caesar," a man of "conspiratorial mentality," one who was "ready and willing to overthrow the republic." The issue of monarchy "still remained viable," Sisson writes; Hamilton, "blinded by power," "simply would not allow even the *germ of a spirit of resistance*" (*italics added*).

To summarize all of the sinuous arguments by which Sisson seeks to prove that the election of 1800 was a genuine and not a "pseudo-Revolution" would require a review of essay length. This also applies to his other contentions, of which the following are representative: that "the political system, in the context of its basic ideological conflicts, remained largely unchanged from the early 1770s to 1800"; that a "style of politics, dominated by faction," not party, "continued through the . . . 1790s"; or that Jefferson's presidential campaign in 1800 was a re-enactment of the "old-style politics" of the Revolutionary War era. Apropos of such arguments, one can only say that some of them are based on baffling exercises in semantics while others are startlingly implausible inferences drawn from highly selective "facts."

Had Sisson confined himself to the proposition that the election of 1800 was a revolution in the sense that it represented "a constitutional change of power without violence and bloodshed, a triumph of reason over armed might," his theme would have been unexceptionable, although perhaps too commonplace to sustain a book of 450-odd pages. As it is, Henry Adams still stands.

JACOB COOKE
Easton, Pennsylvania

RUDOLPH J. PASLER and MARGARET C. PASLER. *The New Jersey Federalists*. Rutherford: Fairleigh Dickinson University Press. 1975. Pp. 256. \$13.50.

With this little volume Rudolph and Margaret Pasler join Richard McCormick, Carl Prince, and Herbert Erskowicz in analyzing New Jersey politics between the time of George Washington and that of Andrew Jackson. Since New Jersey had a strong two-party system and since the state occupied a strategic location between New York and Philadelphia, the subject is an important one. The authors had the chance to examine the shift from the first two-party system to the second and to evaluate the work of other historians on politics during the early national period.

It is unfortunate that the Paslers have not taken advantage of their opportunities. They present evidence to document earlier studies rather than adding much that is new. Like parties in other states, the Federalist party in New Jersey evolved from a faction that managed to win the first Congressional election. Compared with their Republican opponents, Federalists in New Jersey were older, better educated, from better families, and more often engaged in manufacturing. Since their candidates were often from the upper class, the Federalists benefited from deference politics, and as deference faded so did Federalism. It comes as no surprise that opposition to the War of 1812 allowed the Federalists to stage a comeback that year or that the war eventually destroyed the party. It is interesting to learn, however, that Republican merchants outnumbered Federalist merchants and that the national bank and the Jay Treaty had little impact on New Jersey politics.

The Paslers have presented a workmanlike local study, but they have not broken important new ground. Content for the most part to stick to chronology and narrative history, they only occasionally turn to interpretation. Although the volume is well organized and easy to follow, it is not especially well written. There are too many stilted phrases, and there is too much dependence on the passive voice.

DONALD B. COLE
Phillips Exeter Academy

WILLI PAUL ADAMS. *Republikanische Verfassung und bürgerliche Freiheit: Die Verfassungen und politischen Ideen der amerikanischen Revolution*. (Politica: Abhandlungen und Texte zur politischen Wissenschaft, number 37.) [Neuwied:] Luchterhand. 1973. Pp. 394. DM 58.

This illuminating study covers the early years (1773-80) of an age that Samuel Eliot Morison has described as one of the great creative periods of American history. With rare perception of the distinctiveness of American political culture the author leads his reader from the rule of provincial conventions and congresses (chapter 1) through the decision for new constitutions and independ-

ence, through a series of neat sketches of the state conventions, and finally to the meat of his book, an analysis of "rhetoric and reality" in this first period of constitution-making. Though entirely logical, this structure appears on first inspection of the table of contents to promise a static result, something akin to a political science treatment. At the outset, however, the author leaves no doubt about his historical intention and his performance realizes his intent: the sense of movement continues to the final chapter on federalism with its squint toward the Constitution of 1787-88.

This volume is *Ideengeschichte* of the first order, the interplay of ideas and happenings, the interaction between the felt theories of a time and the march of events. Significantly, the epigraph is a Weber aphorism: interests (material and imaginary), not ideas, rule the doings of man, but the *Weltbilder* created by these very ideas often determine the pathways in which the dynamics of interests impel courses of action. The inevitable abstractions in Willi Adams' treatise do not float airily but mesh firmly in reality, in the movement toward independence and the evolving constitutional arrangements as the colonists debate fundamental concerns, each treated in a separate chapter: liberty, equality, property, representation, confederation, and the "general welfare." Moreover, the chapters are tightly knit in their progression toward confederation, the greatest political achievement of the founding fathers (chapter 13).

The rich and distinctive texture of Adams' book sparkles with insights, enhanced by his clean-cut organization. From wide research the author formulates a noteworthy statement (pp. 99-109) on the conceptions of a "republic" and a "democracy" in contemporary rhetoric. At another level he focuses on an anomaly of revolutionary thinking in a concise analysis of the problem of liberty and chattel slavery (pp. 186-90). Here, as elsewhere, his mastery of the secondary literature, which is noted in his bibliographical commentary, informs his conception of the Revolution. This conception is no simplistic schema but one of process, unique in antecedents and experiences, conservative in the Burkeian sense, enlightened in problem-solving methods, but not without ambiguities and inner tensions.

This volume deserves translation in its entirety. In the literature on the American Revolution it will find a place in select company.

AUBREY C. LAND
University of Georgia

FLOYD J. MILLER. *The Search for a Black Nationality: Black Emigration and Colonization, 1787-1863*. (Blacks in the New World.) Urbana: University of Illinois Press. 1975. Pp. xiii, 295. \$10.95.

During the antebellum period, nine-tenths of black Americans were slaves, and free blacks, half of whom were located in the South, suffered intense discrimination and proscription. A major response to these conditions on the part of Northern black leadership was the advocacy of selective emigration, black nation-building, and a pan-black ideology. These themes form the focus of this volume; they have been treated before and are quite well known to scholars. Miller's book, however, has the merit of being the most minutely researched and sharply focused treatment of the subject. The author traces the origins of articulated Afro-American emigration sentiments and interest in Africa among urban New England blacks in the last two decades of the eighteenth century and their growth in the early nineteenth century through the stalwart efforts of Paul Cuffe. The ebb of these feelings in the 1830s resulted from the rise of militant abolitionism and from concerted opposition to the influence of the American Colonization Society, which blacks feared would lead to their forcible deportation. Interest in emigration flowered in the decade before the Civil War under the stimulus of aggravated and intensified discrimination against free blacks and from the leadership of such major figures as Delany and Holly.

Although the author's focus is clear, and though he has shown remarkable diligence in his pursuit of sources, the final product is disappointing. There are, particularly when he is dealing with Africa, too many outright errors, dubious statements, and shockingly anachronistic usages. What is one to make of a scholar writing in the '70s who uses such phrases as "benighted Africans" (p. 26), "degraded peoples of Africa" (p. 38), and "backward heathens" (p. 258) without quotation marks or in any way disclaiming that such are his views? He confuses the names of African peoples with their area of habitation. Instead of writing of the Yorubas of Western Nigeria, for example, he employs the term Yoruba throughout as synonymous with Western Nigeria and writes of the Guinea coast and the Windward coasts without defining them. He is grossly incorrect in stating that in the 1850s the Niger Valley was unknown, that the mouth of the Niger lay undiscovered by Westerners, and that Haiti had a "monarchial" government. The phrase "black Liberians" further shows his weakness in African history.

The author's style is lackluster and sometimes clumsy. There are needless repetitions throughout, and the work is not endowed with much imaginative sympathy. The critical faculty is employed in petty ways; there are frequent references, for example, to alleged contradictions in action and thought on the part of those the author calls nationalist-emigrationists. These individuals could,

more precisely and with little contradiction, be called pan-black nationalists. Not enough appreciation is given to the overwhelming odds they faced and the bold steps they took to meet their enormous problems. In short, this is a useful, workmanlike, but quite flawed volume.

HOLLIS R. LYNCH
Columbia University

EDWIN J. PERKINS. *Financing Anglo-American Trade: The House of Brown, 1800-1880*. (Harvard Studies in Business History, 28.) Cambridge, Mass.: Harvard University Press, 1975. Pp. xi, 323. \$15.00.

The House of Brown, now Brown Brothers, Hariman & Company in the United States and Brown, Shipley & Company in the United Kingdom, was founded in Baltimore in 1800 by the linen merchant Alexander Brown. At first the firm offered customers the usual range of mercantile services; operated on its own account in the buying and selling, importing and exporting, of manufactures and commodities; and extended its business into such ancillary activities as shipowning. To all appearances the House of Brown was no different from a hundred other enterprises along the Atlantic seaboard. While the rapidity of the firm's growth quickly set it apart from competitors, much more fundamental distinctions were less easily seen: early specialization in commodity markets, particularly cotton; active participation in the financing of foreign trade; the establishment of branch offices on both sides of the Atlantic rather than dependence on commission agents; and a managerial concept admitting to the inner partnership circle only those family members who met the highest standards of ability and integrity. By 1830 the House of Brown had surpassed the Barings in capital resources, and after the demise of the Second Bank of the United States it became pre-eminent in the management of the economy through its oligarchic position in foreign exchange markets. Throughout, but especially in the sixty years from 1820 to 1880, the House of Brown used its power to stabilize and regularize the international trading system by participating in the development of the futures market in foreign exchange, by monetizing commercial paper through the telegraph and transatlantic cable, and by contributing significantly to the form and acceptability of commercial instruments. As cautious and conservative as they were in most things, the Browns seemingly acted out of character by speculating in the specie point market. The truth was that their information about and understanding of price movements were so precise that the risks were more apparent than real.

This is an exceptionally fine study of the financ-

ing of foreign trade. Naturally, the Browns dominate the book in the same way they dominated the evolution of financial forms and mechanisms; however, they are studied not for themselves but as a vehicle for analyzing and explaining fundamental economic processes.

PETER J. COLEMAN
University of Illinois,
Chicago Circle

MICHAEL FELDBERG. *The Philadelphia Riots of 1844: A Study in Ethnic Conflict*. (Contributions in American History, number 43.) Westport, Conn.: Greenwood Press, 1975. Pp. xi, 209. \$14.50.

Michael Feldberg has made an important re-evaluation of Philadelphia's notorious Bible Riots of 1844, when pitched battles between nativists, Irish-Catholics, and the state militia left twenty dead in Kensington and Southwark. In the tradition of E. J. Hobsbawm and George Rudé, with a street-wise toughness that marks the latest urban research, Feldberg traces the conflict to the growing rivalry between Protestant working-class and Irish-Catholic "cultural sub-communities," a rivalry sharpened by intense job competition. His Philadelphia is a pre-industrial city with a tradition of artisan radicalism, climaxed by the General Trades' Union's ten-hour movement in 1835, which skilled native craftsmen even extended to Irish day laborers. These attempts to control the labor market were dashed by the 1837 depression and by the thousands of immigrants who flocked to the steam factories in Kensington, Southwark, and other industrial suburbs. The 1840s saw a bitter retreat to an ethnic solidarity. Fire and militia companies, gangs, and parish churches sustained the Irish, while the native-born joined similar but separate institutions, including evangelical churches, temperance societies, and Philadelphia's American Republican party (ARP). Feldberg's socioeconomic analysis of ARP membership and close reading of party manifestos substantiate his argument that American Republicans enjoyed a large artisan following that was attracted by campaigns for free banking, hard currency, and "reform" of immigrant involvement in politics. Where Richard Hofstadter, Seymour Martin Lipset, and other consensus historians saw paranoid, anti-Catholic ravings, Feldberg finds fairly level-headed politics designed to give the aspiring craftsman control over his market conditions.

Feldberg is less convincing in attempts to connect ARP ideology with ethnocultural institutions on the ward level. While the party's anti-Catholic propaganda touched off the confrontation about Bible-reading in Kensington's public schools, the subsequent bloodshed was the work of nativist

gangs, probably indifferent to ARP politics, and bands of teenage rowdies at loose ends between a dying apprenticeship system and a factory discipline not yet in gear. Feldberg adds that after the riots, Philadelphia's business elite considered city-county consolidation, along with professional police, to tame the fire companies and youth gangs. We need to know more about these volatile cobblestone elements.

In this serious, patient study Feldberg has focused on a crucial moment in Philadelphia's transformation into a "modern" metropolis and has brought to it a mastery of the crosscurrents that swept the antebellum city. He has returned students of nativism to a consideration of social structure and substantive grievances, a contribution historians must welcome.

JOEL SCHWARTZ
Montclair State College

JOHN F. REIGER. *American Sportsmen and the Origins of Conservation*. New York: Winchester Press. 1975. Pp. 316. \$10.00.

Reiger's brief text considers American sportsmen and their code, wildlife, the fight for forests, the development of the national park concept, the Boone and Crockett Club, and the establishment of a national conservation policy. The book offers the following dogmas: scholars, "instead of ignoring sportsmen, or treating them with contempt," should acknowledge and investigate their role in motivating environmental reforms; conservation began with wildlife and not with the nation's forests; sportsmen, in time, propelled an interest in forests because of the need to preserve the habitat for wildlife, and they also assumed a leading role in creating and protecting national parks; the Boone and Crockett Club, formed in 1887, and not the Sierra Club was the first private organization to deal with conservation; George Bird Grinnell was the originator and synthesizer of conservation ideas who "prepared" Theodore Roosevelt for Gifford Pinchot.

How new Reiger's interpretation is rests, in part, on his dates for conservation's beginnings. The sportsman-historian prefers the early 1870s, when *American Sportsman*, *Forest and Stream*, and *Field and Stream* provided communication outlets and a group identity for outdoorsmen. Other historians have seen conservation as an unimportant political movement until the twentieth century. Still others have begun with the philosophers of conservation and their works: Henry David Thoreau and *Walden* in 1854; George Perkins March and *Man and Nature* in 1864; W. J. McGee and his report, "The Relation of Institutions to Environment," to the board of regents of the Smithsonian Institution, May 23, 1896.

The newness of the interpretation continues with an expression that the public does not distinguish between sportsmen and other factors that produce species extinction: habitat alteration and destruction, killing by farmers, and commercial hunting. Yet there is evidence that conservation authorities have noted, without distinguishing special categories, that hunters were partly responsible for the tragic loss of wildlife and that they were mainly responsible for attempts to maintain or re-establish wildlife population. Theodore S. Palmer's *Chronology and Index of the More Important Events in American Game Protection, 1776-1911*, for example, could lead one to suggest that 1776 might well serve as the beginning of American conservation and to conclude that the name of the conservation game has been long known, if not too well played, throughout the nation's history.

Of 316 pages comprising *American Sportsmen and the Origins of Conservation*, less than half of the total pagination represents text. Seventy pages are given to a picture album of sport and conservation, which would have enlivened the reading if the isolated cartoons, water colors, and portraits had been appropriately positioned with subject matter. Ninety-three pages are devoted to notes, a select bibliography, and an index. Here, too, some of the annotations might have been more fairly presented if they had immediately followed, or in some instances been incorporated within, the textual material.

WILLIAM T. DOHERTY, JR.
West Virginia University

HARLOW LINDLEY, editor. *Fort Meigs and the War of 1812: Orderly Book of Cushing's Company, 2nd U.S. Artillery, April, 1813-February, 1814 and Personal Diary of Captain Daniel Cushing, October, 1812-July, 1813*. Columbus: The Ohio Historical Society. 1975. Pp. xiii, 146. \$3.00.

This is a new edition of the orderly book and personal diary of Captain Daniel Cushing. It is mainly concerned with Fort Meigs during the War of 1812. The first publication was in 1944 as volume 11 of the *Ohio Historical Collections*. Since the original publication attracted little notice among historians and is now quite scarce, this new publication is of interest. A bonus is the addition of several maps drawn by officers serving at the fort.

Following various American defeats in the west during 1812 and early 1813, Major General William Harrison realized American success would necessitate a strong western base. That is the significance of Fort Meigs; far enough from the British to discourage casual raids, near enough to the overland supply trails from Ohio, and strong enough to serve as an assemblage and deposit point for potential offensive operations.

Like many among the newly augmented federal officer force, Cushing accepted a reduction in rank from a brigadier general of the Ohio militia to become a captain of the regulars, in his case the Second Artillery. The orders and diary include the period of the British siege of April and May 1813 and the abortive attack of July. In short, this was the period of the first real success for the United States in the West.

ALEC R. GILPIN

Michigan State University

MAJOR L. WILSON. *Space, Time, and Freedom: The Quest for Nationality and the Irrepressible Conflict, 1815-1861*. (Contributions in American History, number 35.) Westport, Conn.: Greenwood Press. 1974. Pp. x, 309. \$13.95.

Relying "primarily on the congressional debates and presidential messages" of the antebellum period and dismissing any sharp distinction between "rhetoric" and "reality," Major L. Wilson has attempted to write an intellectual history of American nationalism and the coming of the Civil War by relating the conceptual elements of space, time, and freedom to the subject.

The book is richly studded with arresting quotations. Most of them reinforce Wilson's argument that Federalists and later Whigs saw the Union in a process of continuous reconstruction; the United States was to experience qualitative improvement through time as it moved across unsettled space. In Wilson's lexicon this is the idea of "corporate freedom," more familiarly known as positive liberalism. Jeffersonian Republicans and Jacksonian Democrats on the contrary were, according to Wilson, advocates of a "federative freedom"—state rights and negative liberalism—assuming that freedom was perfect at the creation of the nation and calling, over time, for quantitative improvement, that is, the extension of the area of freedom.

Out of this dialectical process of conflicting interpretations of the concept of freedom there came the idea of "freedom national"—that individual freedom alone, without slavery, should give form to the new and unsettled part of the Union and ultimately to the entire nation. "Freedom national" was defended as a form of negative liberalism; slave power took the place of state power, the abandonment of which would allow freemen to go on to "unplanned glory." The alliance between "plain republicans" of the North and Southern slaveholders under the umbrella of federative freedom broke down; "freedom national" versus "slavery national" transcended the older debate between corporate freedom and federative freedom, making the Civil War inevitable.

These crucial parts of Wilson's argument are not altogether clear and not entirely persuasive. The

methodology is at fault here: the quotations are never demonstrated to be representative of the individuals or leaders cited; the analysis is narrowly political, with only scant attention given to the works of Lee Benson, Joel Silbey, and Edward Pessen. People's daily behavior, the dynamics of the economy, ethnocultural affiliations and tensions, and religious ideology play little or no role.

While we can agree with Wilson that ideas are "a part of reality" and "have a life of their own," we must severely fault him for failing to attempt to relate political ideology to social context. Ideas are critical to historical analysis because they are ultimately what people act on, but they can no longer be viewed simply as independent entities with no connection to economic or social setting.

Wilson's dialectical framework that the thesis of corporate freedom (Whigs, American system, time) versus the antithesis of radical federative freedom (nullifiers, extreme laissez faire, space) equals the synthesis of Jacksonian Democracy (negative broker state) and, later, republicanism (the more positive broker state: aid, but no direction), is useful, ingenious, even brilliant, but it does not go beyond his 1967 article on the same subject and ultimately fails to explain the alleged irrepressibility of the sectional conflict.

GERALD SORIN

State University of New York,
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HERMAN J. VIOLA. *Thomas L. McKenney, Architect of America's Early Indian Policy, 1816-1830*. (Sage Books.) Chicago: Swallow Press. 1974. Pp. xii, 365. \$15.00.

RONALD N. SATZ. *American Indian Policy in the Jacksonian Era*. Lincoln: University of Nebraska Press. 1975. Pp. xii, 343. \$12.95.

ROBERT A. TRENNERT, JR. *Alternative to Extinction: Federal Indian Policy and the Beginnings of the Reservation System, 1846-51*. Philadelphia: Temple University Press. 1975. Pp. ix, 263. \$15.00.

These three carefully researched, narrowly conceived monographs begin in defense of their protagonists' Indian policies. Each then tells a story of failure. Thomas McKenney, chief administrator of Indian affairs from 1816 to 1830, was the major official spokesman for civilizing Indians. This "sincere humanitarian," in Herman Viola's words, "did his best to maintain a balance between national desires and national honor by just treatment of the Indians" (p. xii). Insufficient control over both Indian culture and expanding, acquisitive frontier society led McKenney to abandon hope for civilizing Indians in the East and to support Indian removal west of the Mississippi. McKenney did not contemplate forced removal, however, and

his dismissal by Andrew Jackson signified the new president's intention to drive the southern tribes west despite their own desires.

Ronald Satz begins with Jacksonian removal. Denying Jackson's historic, personal stake in Indian dispossession, ignoring the illegality of extensions of state law over the southern tribes, and exposing the political motives behind National Republican Indian sympathy, Satz initially defends removal. But its implementation, he argues, was disastrous. Removal against tribal consensus meant fraudulent treaties and military force. Removal by private contractors and an economy-minded government meant corruption, starvation, and Indian death. Removal manipulated by sale of individual Indian land allotments made the Choc-taws and Creeks "victims of one of the most flagrant cases of fraud, intimidation and speculation ever recorded in American history" (p. 83). Removal to make room for land-hungry eastern whites failed to guarantee Indians their newly promised land against expansionist pressures in the West. To remove Indians from the midst of a society of "expectant capitalists," writes Satz, was "an acceptable and desirable policy" (p. 54). But since the character of that society explains the character of removal, the distinction between policy and implementation fails.

For the administrators who followed removal, and their historian Robert Trennert, Jacksonian policy was too *laissez faire*. It left western Indians at the mercy of frontier traders and settlers. Efforts to reassert government control, Trennert suggests, led to the reservations policy: confining, controlling, and protecting Indians, in the face of Manifest Destiny, was the alternative to extinction. But postbellum implementation of the reservations policy, Trennert indicates, reproduced the failures of McKenney's civilization and Jackson's removal. "Brutal and disastrous" reservation confinement limited the tribes to smaller and poorer lands (p. 196). These consequences, Trennert insists, were unimagined by originators of the policy.

This lack of imagination, reproducing illusions shattered a decade earlier, was itself an achievement. Policy makers thought the tribes would passively accept white plans because they failed to see Indians as human agents, with purposes and cultures of their own. When McKenney evoked "their helplessness and their dependence on [the president] as their father" (p. 37), his intention was more than descriptive. He wanted to make Indians over into the "children" (p. 176) he described. Indians were victims not just of expectant capitalist society but of a paternalist state.

Viola, in spite of his sympathy for McKenney, pictures a "patronizing and self-satisfied man" (p.

144) who longed for power over his Indian children, opposed Cherokee literacy in the native language, "delighted in tearing apart the fabric of Chippewa culture," and busily rescued Indian relics from, in McKenney's words, "the ultimate destruction which awaits his race" (p. 243). McKenney, according to Viola, also turned the Indian plight to his own promotion and profit. Satz defends Jackson's "great paternalism" (p. 9) early in his book, but he ultimately recognizes that "the paternal hand of the United States . . . undermined tribal power" (p. 126-27). The civilization program in the West, he writes, "reduced the Indians from cultural maturity as members of tribal societies to cultural infancy as civilized men" (p. 277).

Satz shows that Jackson sacrificed promises of Indian autonomy in the west to government political and social control. Trennert is thus wrong to suggest that the reservation policy of "benevolent paternalism" (p. 1) departed radically from a *laissez-faire* removal. Like Viola, Trennert defends "the sincerity of the humanitarian concept" (p. 194). The reservation policy was "ethnocentric" (p. 1), to be sure, but this assertion functions as apology. Nineteenth-century humanitarians assumed that Indians were less than adult humans, and they formed their schemes for Indian betterment accordingly. Trennert insists that the twentieth century not sit in judgment on widely shared assumptions of the nineteenth.

Here the power of positive historical thinking weakens all three of these books. Cultural symbols have histories, too. Nineteenth-century ethnocentrism was not a given but a human project, serving functions for society and the psyche in red, white, and black America. Interpreters of Indian policy must learn from recent studies of white over black to penetrate racial ideologies and not accept them at face value. The alternative is positive history, exculpatory and ahistorical. To be bound by the world view of policy makers, in the name of historical objectivity, is not to think historically.

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Berkeley*

RUSH WELTER. *The Mind of America, 1820-1860*. New York: Columbia University Press. 1975. Pp. xvi, 603. \$14.95.

Rush Welter's study of the ideology of antebellum United States suggests the resilience of an older tradition of intellectual history recently eclipsed by the profession's greater social emphasis. Despite claims about writing from "the often inchoate ideas" of "relatively ordinary men and women,"

Welter explores the "American mind" through the words of social and political leaders. The "ordinariness" of the American women quoted (once each)—Catherine Beecher, Angelina Grimké, Julia Ward Howe—is not much different from that of the men. Politicians, with emphasis on those active in state constitutional conventions, ministers, college figures, and social publicists and theorists make up the bulk of the book's sources. If the sources are not different in kind—except for the exclusion of literary ones—from those normally used, they are uncommonly various. One value of the study is as a treasury of quotations from neglected materials.

Rush Welter centers his argument on the conventional dichotomy: liberal or radical Jacksonians versus conservative Whigs. Many of the study's weaknesses are rooted in this structure, especially in the repetition of alleged ideological opponents making identical arguments, and in its tendency to let categories control ideas. A four-page footnote pretending to distinguish Whigs from "conservative" Democrats represents the book's faults at their worst: lengthy quotations that illustrate little—certainly not the distinctions Welter insists on. The book avoids the worst fault of the political-ideological tradition Welter cultivates, the good guys versus the bad guys syndrome. Welter analyzes with sufficient critical vigor, so that both sides turn out to be composed of mistaken, if not bad, guys.

Welter sees Jacksonian ideas as grounded in a concept of "equal rights," which meant hostility to government favoritism or interference. Welter does justice to the liberal impetus and conservative conclusion that grew from Jacksonian hostility to government "privilege" and the toleration of economic and social distinctions deemed "natural" rather than "artificial." Other aspects of his Jacksonian description are less convincing, especially his stress on the party's political "Calvinism" or "fatalism." The antigovernment bias of Jacksonians owed much more to abstract eighteenth-century notions of a just, natural order than to a Calvinistic acceptance of pervasive evil. It was, as the *Democratic Review* said, "a cheerful creed," weakened more by naive hopefulness than any sense of grim human inevitabilities.

The Whigs' argument is less interestingly explored. Their central tenet, Welter says, was a perception of property rights that moved from an organic conception of property as subservient to the social good to a notion of its absolute sanctity. The case, at best, is simplification; that Welter is wrong in asserting that "when a conservative spoke of rights during the 1840s, it was an all but foregone conclusion that he had in mind the rights of property," is indicated in many quotations in

the book and in the author's own later discussion of civil liberties. Nor does he present any real evidence for his view that the Whigs gave up "the vision of society as an organic unity," although democratic sensibility led them increasingly to define this in promotional or reformist rather than paternal terms.

The gist of Welter's criticism of both parties is that their separate ideas led to a joint result: a fear of government power that paralyzed positive action. This conclusion loses much of its bite because Welter recognizes little danger in power. His discussion of Jacksonian views of limited government or Whig emphases on constitutionalism and minority rights show little response to the fact that power in modern societies has often been as destructive as it has been constructive. Welter gives little heed to Democratic stress on the sanctity of majority and individual power, and the way that these themes accentuated Whig concern for both natural rights and legalism as protective devices.

The author incorporates chapters on American views of history, church, school, and frontier that share the substantial virtues and lesser faults of the specifically political material. Themes are often pushed to misleading lengths. His first chapter insists that Americans believed "that the United States had already fulfilled the progressive dreams of mankind," but the material suggests the more moderate belief that the American Revolution had laid a foundation which, if properly built on, could support a better society than any previously known. Throughout these chapters the book suffers from an abstraction from the social and political realities that gave ideas life. Welter chides Eric Foner for "his tendency to render ideas as the expressions of practical interests or as instruments of Republican polemics," but Foner's emphasis on the way ideas are shaped by, as well as shape, practical reality gives his account of the coming of the Civil War a social and psychological credibility lacking in Welter's work.

Whatever its faults, Welter's study has a breadth, thoughtfulness, and richness of research that commend it to anyone seriously concerned in making sense of the tangled ideological-political web of antebellum United States.

DAVID GRIMSTED
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College Park*

SHERRY PENNEY. *Patrician in Politics: Daniel Dewey Barnard of New York*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. xv, 206. \$12.50.

This traditional biography of Daniel Dewey Barnard traces the response of a patrician Whig politi-

cian to the changing political and social landscape of the Jacksonian era. Born into wealth, Barnard was trained as a lawyer, served four times in Congress from upstate New York, was a chief contributor to the *American Whig Review*, and headed the American mission to Berlin during Fillmore's administration.

From his first involvement in politics until his death in 1861, Barnard maintained a conservative philosophy that included a belief in slow, orderly growth regulated by a political leadership composed of superior men of intellect, ability, morality, and wealth. He disdained popular mass politics and had a passionate interest in preserving the Union.

Sherry Penney explains Barnard's actions, however inconsistent, in terms of his devotion to these beliefs. For instance, although he vigorously opposed social reformers, Barnard was a fervent supporter of educational reform. He argued for state funding of common schools in order to teach the masses due respect for order, stability, and the proper virtues. Despite ruminations about the horrors of mass politics which kept him at odds with seasoned politicians within his own party, Barnard campaigned for Harrison in 1840 and urged that patronage be used whenever possible to satisfy the Whig interest—not simply to pay off political debts but to place in power principled men like himself. Not surprisingly, Barnard vigorously opposed Andrew Jackson and the Democrats. Dedicated to Clay's American system, he saw Whiggery as the best hope for America's future. Yet in the 1850s Barnard supported Democratic candidates in an effort to destroy the Republican party and preserve the Union.

The conceptual framework employed in this study is hardly novel. Undue emphasis is also accorded Barnard's perceptiveness and influence. To note, for example, his prediction in the 1850s that the Civil War was inevitable as an illustration of the man's prescience is not convincing. Nevertheless, Penney's account helps illuminate the reactions of a certain class of individuals to the convulsions of antebellum America.

PETER LEVINE
Michigan State University

GILBERT GIDDINGS BENJAMIN. *The Germans in Texas: A Study in Immigration*. Austin, Tex.: Jenkins Publishing Company. Reprint. 1974. Pp. 159. \$12.95.

While a doctoral candidate at Yale between 1904 and 1907, Gilbert Benjamin researched and wrote *The Germans in Texas*. The work was originally published by the University of Pennsylvania Press in 1909; the present reproduction is by photo-offset with only the title page having been replaced.

The decade before World War I was not only famous as the most expansive years in American immigration history, but it was also an era in which sociologists, economists, and special investigators attempted to explain the phenomenon. A score of influential and sometimes controversial accounts were published. Those by Prescott F. Halls, Edward A. Steiner, John R. Commons, Jeremiah W. Jenks, Peter Roberts, and Henry P. Fairchild were read not only by scholars, but also by government officials, congressmen, and the general public. Benjamin, however, was little influenced by these widely discussed and popularized sociological studies. The era also became known for a growing filiopietism. One of the more famous pro-German books was Albert B. Faust's massive two-volume account, *The German Element in the United States*, published the same year as *The Germans in Texas*. In his Texas study, Benjamin made no special claims for the German migrants and did not reflect the nationalistic bias prevalent in many immigrant stories. By the turn of the century a number of literary observers were using fiction to provide a meaningful insight into America's immigrant heritage. But even the repeated appearance of the Teuton in works like those of Sidney Lanier, William Dean Howells, Henry James, and Berthold Lindau seemingly had limited impact upon Benjamin's historical evaluations.

In short, *The Germans in Texas* is a detailed and factual historical monograph, a brief and somewhat restricted account of both individual and group settlement during the first half of the nineteenth century. Benjamin surveys the German response to slavery, secession, Reconstruction, and the Indians, and their contributions in establishing newspapers, singing societies, literary clubs, and churches is surveyed. While not engagingly written or philosophically expansive, the study is carefully researched, historically balanced, and nonpropagandistic. It has been used as an authoritative reference by immigrant scholars (Carl Wittke often refers to Benjamin) and perhaps will find a new audience with the current revival of German festivals in Texas.

Unfortunately, this reproduction provides no introduction, updating, editorial comment, or evaluation of the book, the author, or recent immigrant studies on Texas settlement. A brief note on the dust jacket declares the work to have been "researched at a time when many of the original leaders and participants were still alive." After nearly seventy years *The Germans in Texas* deserved a more explanatory and reflective second launching.

WILBUR S. SHEPPERSON
*University of Nevada,
Reno*

MARK E. NACKMAN. *A Nation Within A Nation: The Rise of Texas Nationalism*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. 183. \$12.50.

In this work, Mark Nackman maintains that Texas remains a "nation within a nation." A strong and persistent sense of identity was generated by the common hardships of the republican period. Indian harassment and the specter of Mexican reconquest characterized this era, yet many Texans viewed annexation with reluctance. Exaggerated claims to Santa Fe and renewed interest in the republic as an alternative to secession were other manifestations of this nationalism. Nackman concludes that the annual celebration of Texan independence, the annual proclamation of Texas History Week, and the ever-present symbol, the five-pointed "Lone Star," indicate that the virus is still rampant.

Nackman is correct when he emphasizes that since most immigrants were fleeing economic ruin and later prospered in Texas, they developed a fierce attachment to their adopted land. Like all who write about Texas history in the early period, the author is intrigued by the high suicide rate among persons of prominence, and he speculates on the possible "death-wish" of Travis at the Alamo and Fannin at Goliad. Their martyrdom and that of the men who willingly followed them also fostered a powerful sense of patriotism and cohesion among Texans of later generations. The author is perceptive when he observes that nationalism has never had much attraction for the black and Mexican residents of Texas.

On balance, this is a worthwhile book, but the writer's conclusions are labored and overstated, and furthermore, I do not believe they are peculiar to Texas.

STANLEY E. SIEGEL
University of Houston

LUCILLE WRUBEL GRINDHAMMER. *Art and the Public: The Democratization of the Fine Arts in the United States, 1830-1860*. Stuttgart: J. B. Metzlersche. 1975. Pp. 86. DM 20.

In this brief, 25,000-word monograph, published in Germany but based on her American dissertation, Lucille Wrubel Grindhammer surveys the various methods employed to popularize the fine arts in antebellum America. She focuses on "the newly emerging mass media," the illustrations and articles that publicized artistic achievements in newspapers, magazines, gift books, and fiction, as well as on the work of the immensely popular art unions, which distributed engravings and original art in the 1840s and 1850s. Another sign of spreading interest, according to Grindhammer, was the creation of art and drawing classes in American

schools and colleges; the "educational efforts did turn around the culture's formerly negative attitude toward art." She concludes that the origins of contemporary mass culture can be found in the Jacksonian era and in the encouragement that printed media gave to hundreds of American painters, sculptors, illustrators, and engravers.

The study brings together much useful information and demonstrates bibliographic mastery. However, the conclusions present little that is new, and the brevity of the volume forces a certain level of reductionism into the cultural analysis. That periodicals, publishers, and other patrons supported and stimulated American artists is undeniable, but one wishes to know whether they increased understanding of or sympathy with esthetic philosophies and techniques, whether they developed a permanent mass clientele for the fine arts, and whether they truly originated a mass culture, for mass culture is structurally and functionally quite different from the popularization they aided. Grindhammer is less successful in answering these questions. Finally, there are some errors in the references to artists, including a series of misspellings.

NEIL HARRIS
University of Chicago

NORMAN D. BROWN. *Edward Stanly: Whiggery's Tarheel "Conqueror."* (Southern Historical Publications, number 18.) University: University of Alabama Press. 1975. Pp. viii, 365. \$10.00.

Arthur Charles Cole called him "one of the ablest champions of whiggery in Congress," but North Carolina's Edward Stanly, who served ten years in the House of Representatives from 1837 to 1853, gained his greatest fame in that body as a result of his personal disputes—sometimes verbal and sometimes physical—with fellow lawmakers, Democrats and Whigs alike. In 1851 he engaged in "the last duel in the United States arising out of congressional debates."

As a congressman, Stanly was difficult to categorize. Despite his "occasional gasconading" on the subject of slavery, his enemies labeled him "a Southern man with Northern principles" because of his inconsistent stand on the gag rule and his support of the Tariff of 1842. He was one of two Southern Whigs in the House who supported President Zachary Taylor against Henry Clay's Omnibus Bill in 1850, yet he ultimately voted for more individual compromise measures than any other North Carolinian in Congress. Gerrymandered out of his seat by a Democratic legislature, he moved to San Francisco in 1853. His departure for California occurred at a time when the Whig party was fast disintegrating, and he never again found a political organization in which he felt completely at home.

Stanly's most controversial political role was as President Abraham Lincoln's military governor of North Carolina from May 1862 to March 1863. He faced an impossible task, for he was charged with establishing a pro-Union government in the eastern part of the state during "the high noon of Confederate military success." On the one hand, old friends and relatives regarded him as a traitor; on the other, Northern abolitionists accused him of seeking to perpetuate the institution of slavery. The issuance of the Emancipation Proclamation, which he believed made his mission hopeless, prompted his resignation.

Norman D. Brown concludes that Stanly cherished "three abiding loyalties—North Carolina, the Whig Party and, above all else, the Federal Union." It is unfortunate that the biographer has presented only an incomplete picture of the man, for his diligent search for Stanly letters in the manuscript collections of his contemporaries did not adequately compensate for the complete lack of family and personal correspondence. Brown's study is at its best in dealing with those phases of Stanly's career in which he figured prominently in the political arena. But at times "Whiggery's Tarheel 'Conqueror'" appears merely as a shadowy individual offering occasional opinions on the stirring events of his time.

EDWIN A. MILES
University of Houston

ton's Farewell Address. Furthermore, his ambiguity left his listeners uncertain of his commitment to republicanism and his attitude toward slavery.

Komlos' study, originally presented as a master's thesis at Northeastern Illinois University, would be well received by any graduate faculty. As a book, however, it retains too many of the flaws common to master's theses. For a subject that is inherently colorful, the writing style is dry, overburdened with unnecessary quotation and documentation, and marred by errors of grammar, syntax, and citation. The work also lacks emphasis; all the facts of Kossuth's journey are elaborately set forth without consideration for their relative significance. The East European Institute of the State University College at Buffalo, under whose auspices the volume appears, owed the author stronger editorial guidance.

Kossuth found to his own disillusion that Americans' wild acclaim was not easily translated into concrete assistance for his future plans. Most were simply seizing upon him as a convenient symbol by which to celebrate themselves. The ideological and emotional undercurrents of America's response are not deeply touched upon by Komlos and continue to offer an inviting subject for exploration.

JAMES M. BERGQUIST
Villanova University

JOHN H. KOMLOS. *Louis Kossuth in America, 1851-1852*. Foreword by C. A. MACARTNEY. (Program in East European and Slavic Studies, number 4.) Buffalo: East European Institute. 1973. Pp. 198.

Few foreign visitors to the United States have evoked a welcome as spectacular as that accorded the Hungarian revolutionary Louis Kossuth in 1851 and 1852. Yet the event has not been dwelt upon very much by American historians, few of whom have been able to use the massive work in Hungarian by Dénes Jánosy. By providing in English a detailed narrative of Kossuth's tour and an appraisal of its political and diplomatic implications, John Komlos has made a welcome contribution.

Komlos harbors no illusions about Kossuth, finding him largely responsible for the failure of his own American efforts. There was never, of course, any prospect of official help from the American government. But Kossuth himself spoiled whatever chances there were for extensive private aid for his cause. He appears as a poor leader who was hampered by his own vanity, whose ineffective organizing frittered away what meager funds he gained. He lectured in condescending fashion to Americans about the proper interpretation of Washing-

MILDRED THRONE. *Cyrus Clay Carpenter and Iowa Politics, 1854-1898*. Iowa City: State Historical Society of Iowa. 1974. Pp. xi, 302. \$8.00.

For more than a decade historians have awaited the resumption of publication by the State Historical Society of Iowa of those special scholarly studies and biographies for which the society had become justly respected. To signal the return of these publications the society has published an almost forgotten manuscript on the life of Cyrus Clay Carpenter by the late Mildred Throne. Long esteemed by her colleagues as an excellent editor and able scholar, Throne has, in effect, produced her own splendid memorial.

Devoted to the Republican party since its birth, Carpenter was a minion of the Dodge-Clarkson faction of the party that controlled politics in Iowa for more than a generation after the Civil War. Yet, he remained on good terms with such antimachine Republicans as James Grimes, James Harlan, and John A. Kasson. A reliable Radical, indefatigable campaigner, and trusted party work horse, Carpenter's rewards for faithful service included two terms as governor, two terms in Congress, and an appointment as a minor functionary in the Treasury Department. As Throne so ca-

pably points out, Carpenter lacked the necessary character traits essential for attaining the front ranks of political leadership, and therefore he always remained a minor figure in political circles, more respected than admired. Throne's fine study has rescued Carpenter from an obscurity that some may believe he correctly merits. Her balanced presentation shows competent scholarship, judicious restraint, and felicitous style. This volume complements earlier works on Carpenter's political contemporaries, Younger's *John A. Kasson* and Sage's *William Boyd Allison*, both also published by the State Historical Society of Iowa.

A number of weaknesses are apparent in the book, perhaps because Throne did not believe her manuscript was quite ready for publication. We look in vain, for example, for a thorough examination and penetrating analysis of the origins and development of the various splinter parties in Iowa during the last third of the nineteenth century; of the roles of the silver issue and the tariff question in local politics; of the depth and extent of the power of the railroads in the state. These are, at best, only noted superficially. Nevertheless, this biography makes a useful addition to the historical literature of late nineteenth-century Iowa.

MORTON M. ROSENBERG
Ball State University

GEORGE HARWOOD PHILLIPS. *Chiefs and Challengers: Indian Resistance and Cooperation in Southern California*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xi, 225. \$10.95.

This is a well-researched, lucidly written monograph on southern California Indians. George Harwood Phillips revises the writing of American history by according the Indians the attention they deserve in their relationships with the white man. Consequently, Phillips does not dwell on the mistreatment of the Indians by Americans and does not view the whites "as the elites of American history." Instead, he examines how the Indians responded to the Spanish, Mexican, and American intruders "in ways that were logical and valid in light of their own experiences." By doing this he demonstrates that the southern California Indians were important in molding the history of the region to the 1860s.

In this study of Indian resistance and cooperation, Phillips focuses on the ambivalent responses of the three most active Indian groups whose lifestyle was affected by the American conquerors. The Cahuillas, led by Juan Antonio, responded by being cooperative with the Americans and Californians but received the least consideration and material help from them. The Cupeños, headed by Antonio Garra, who sought to organize an Indian

confederacy, rose against the Americans at Warner's Ranch and Kupa. Although the Garra or Cupeño uprising struck fear among the English-speaking population and made Garra a wanted criminal, it was Juan Antonio who turned him over to the authorities. The Luiseños were directed by Manuelito Cota, an American appointee and collaborator. Unlike Juan Antonio, who died in poverty, and Antonio Garra, who was executed, Manuelito Cota prospered and enjoyed old age.

Although I can understand the circumstances that led the chiefs to respond differently, I disagree with the author, who seems to be more appreciative of Cota's and Juan Antonio's leadership than of Garra's. Notwithstanding this difference of opinion, Phillips must be congratulated for producing a model of how United States history should be written and for making an original contribution to a long-neglected facet of California history.

MANUEL P. SERVIN
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SHELBY FOOTE. *The Civil War: A Narrative*. Volume 3, *Red River to Appomattox*. New York: Random House. 1974. Pp. 1,106. \$20.00.

"Farwel my book and my devocion." With words of Chaucer, Shelby Foote completes his massive three-volume study of the Civil War. Thus ends a twenty-year love affair of an author with words and colorful descriptions of men and events in our great sectional conflict, leaving us richer in new literature and historical narrative of that war. This work will surely rank with the classic studies of Allan Nevins, Bruce Catton, and Carl Sandburg.

It is with "characters and incidents" of the war that Foote is at his best. "I have searched them out," he writes. "I took them as they were." With creative imagery, he sets his characters upon the stage of war—soldiers and civilians, the nameless and renowned, the hero and the knave—and each responds to war's complexity in his own particular way, be it "the scantily clad Confederates and their crowbait nags," blue-coated Yankees dying recklessly before Cold Harbor, or generals and civilians behind battlelines, in endless conferences that determined fates of nations—and of other men.

Foote's portrayal of Robert E. Lee is a masterpiece of understanding and love, capturing the distinctive qualities of a Southerner he never ceases to admire. He also recorded the complexities of other lives—the embattled presidents with their sad eyes, generals in victory and defeat, and the lesser-known figures who also served. There is always the lingering regret for the fallen, the gnawing wonder of what might have been had

the statistics not claimed their Jacksons, Stuarts, Reynolds, or Sedgwicks.

On every page is the sound of battle, echoing across the years to the present, making each succeeding generation "a part of" a war that yet defies our imaginations. As in his first volumes, Foote is at home in the field, amidst the surge of armies, the clash of battle, the charging echelons of Blue and Gray fighting at the pinnacle of life to capture another enemy position. There is, however, a growing maturity with each new volume, a style that remains warm and consistently human. The conclusions in the final chapter are refreshing and personal. This account is a mosaic of war—its ferocious charges, the bloody withdrawals, the unrelenting decisions of command, the boredom and loneliness under silent guns, infuriating moments of indecision and disobedience, the exuberance of victory, and the tears of defeat. There is no fatal bias, but as Catton, Nevins, and Sandburg chose Northern perspectives, so Shelby Foote chose a Southern one.

The author depended heavily upon the secondary materials of the war for his research, mining them extensively and thoroughly in his search. He used traditional judgments of other historians to draw conclusions on debatable issues of the war, and he depended too heavily upon controversial interpretations such as Hudson Stroud's characterization of Jefferson Davis and Charles Ramsdell's interpretation of Civil War causation.

Though history written as literature may cease to be history as we should define it, it may still retain validity as an art form and as another source. Foote's monumental work may lose some of the objectivity that goes with more up-to-date research, but these volumes are commendable materials for Civil War study. By following different routes to his conclusions, the author has achieved a work of art, one characterized, as the late Wendell S. Stephenson wrote, "by serious purpose and hard work."

In the midst of the celebration of our nation's Bicentennial, this work strips a bit of the luster from our self-congratulations and reminds us again of the dark star that led us down the trail of tears to sectional war. It also reminds us that history can be presented with beauty and feeling and be written for understanding.

ROBERT HARTJE
Wittenberg University

PETER J. PARISH. *The American Civil War*. New York: Holmes & Meier Publishers. 1975. Pp. 750. Cloth \$25.00, paper \$12.50.

The American Civil War has always had great appeal to Britishers. One need only recall the dis-

tinguished works of such authorities as J. F. C. Fuller, Colin Ballard, Liddell Hart, and William R. Brock to be reminded that some of the most engaging and perceptive writing about America's greatest conflict has come from the pens of British authors. *The American Civil War* by Peter J. Parish, lecturer at the University of Glasgow, is an impressive addition to the shelf of distinguished volumes by foreign authors.

Parish's volume is outstanding for its comprehensiveness. It is a big book, with a text of well over 300,000 words, supplemented by twenty pages of reference notes, a chronology, and a remarkably good bibliographical essay. Narrative, notes, and bibliography evidence a thorough acquaintance with published materials treating the Civil War, including general accounts, source collections, biographies, studies of campaigns, monographs, and scholarly articles. Five of the book's twenty chapters deal with the background and causes of the conflict, and the two final chapters are devoted to Reconstruction.

The author maintains a good balance in attention devoted to the North and South; social, economic, and political aspects; domestic and foreign affairs; and military and naval activities. From beginning to end he manifests a desire to be fair.

Parish examines and evaluates various influences contributing to the outbreak of hostilities between the North and South, but he rates slavery as "the prime cause of the Civil War." In evaluating leadership he gives Northerners a higher rating than Confederates. His treatment of Jefferson Davis is sympathetic, but he concludes that much of the trouble experienced by the Confederate president was of his own making. He finds Davis "fixed in his ways, conventional in his outlook, reluctant to forgive a critic or forget a grievance . . . legalistic and self-righteous . . . never able to reach out to the whole Southern people, to articulate their war aims in words of genuine inspiration." Parish is by no means uncritical of Lincoln, but he praises the Union president for his flexibility, resilience, strategic wisdom, political astuteness, courage, and magnanimity. "Of all democratic leaders of his or any other day," he states, Lincoln "best preserved the common touch. . . . Although no one felt the agony and anguish of war more deeply than Lincoln, he also possessed the iron resolution of a great war leader. He sustained the Union cause in its darkest moments. . . . He became, warts and all, the living embodiment of that cause, whereas Jefferson Davis was more of a marble monument to his."

The superiority of the North's political leadership was not confined to the presidency. According to the author, "there were no Confederate equivalents to Seward, Chase and Stanton at cabi-

net level." He characterizes Alexander Stephens, the vice-president, as "a tiny, sickly, neurotic but extremely gifted man." He concludes that as a group, Northern congressmen were more able than their Richmond counterparts. Among Southern governors he finds no one as capable as Morton of Indiana, Andrew of Massachusetts, or Curtin of Pennsylvania. "The South, setting out to create a new government, proved too inflexible, traditional, negative and sterile."

Parish recognizes the "battlefield brilliance" of Lee, Jackson, Forrest, and other Confederate generals, but he concludes that "in military as in political leadership they [the Northern generals] showed a resilience, a flexibility and a breadth of comprehension which the South could not match." For all their brilliance, Lee, Jackson, and their associates "gradually came to look old-fashioned . . . the last great exponents of a dying school, they won the kind of battles which were not to decide this kind of war."

In the first years of the conflict Grant, Sherman, Meade, and other high-ranking Union leaders were less impressive than their opponents, but they grew with experience until by 1864 they and the command system that they helped develop proved unbeatable. "Lincoln and Grant were right to attach so much importance to elimination of the opposing armies," the author states, "but Sherman's mind, like his army, penetrated more deeply in its insistence that in a war of peoples, the prospect of defeat must be brought home to the people themselves." Parish characterizes Sherman's march through the Carolinas as "one of the truly stupendous feats of the war."

In discussing the outcome of the conflict, the author states that the North's enormous superiority in men and material resources was offset to a large extent by the fact that it had the more difficult task of invading and occupying a vast area and conquering a resolute and resourceful people. He aptly states that "more recent wars have underlined the difficulty of converting an overwhelming preponderance of power into final and total victory."

The North's advantage in men and materiel, according to the author, was greatly strengthened by other assets, among them a more diversified economy, a greater capacity for organization, and a more enduring will to win. He states that "the North had a better cause, the greater ideological strength, the less damaging moral flaws." He concludes that the South's internal weaknesses, including an undue attachment to state rights, a stubborn insistence on individual freedom, and an exaggerated idea of the importance of "King Cotton," contributed immeasurably to Confederate defeat. "Above all," he states, "slavery, the very

cornerstone of the Confederacy, proved to be its severest handicap." Slavery, he adds, "blocked the path to European recognition. Perhaps most insidious of all . . . was its corrosive effect on Southern morale and self-confidence."

The American Civil War is remarkably free of factual error. While I find myself in agreement with nearly all of the author's interpretations, I question his apparent conclusion that Northern morale sank so low in 1864 as to greatly imperil the survival of the Union. Letters of Union soldiers indicate such strong and persistent devotion to the Northern cause as to make unlikely the acceptance of Southern independence by the masses, even if Lincoln had failed to be re-elected. I also doubt that Sheridan "had no superior in the Civil War leadership on the field of battle." I would rate Forrest and Cleburne above "Little Phil" as combat commanders. But these are matters of judgment, and the author's views may have more validity than mine. His book is a model of excellence in every respect. In my opinion it is the best of all one-volume surveys of the American Civil War, and it contains more of substance and soundness than many multivolume treatments of the subject.

BELL T. WILEY

Agnes Scott College

DAVID COE, edited and with an introduction by. *Mine Eyes Have Seen the Glory: Combat Diaries of Union Sergeant Hamlin Alexander Coe*. Rutherford: Fairleigh Dickinson University Press. 1975. Pp. 240. \$10.00.

In August 1862 a twenty-two-year-old wagon maker named Hamlin Alexander Coe joined the Nineteenth Michigan Volunteer Infantry Regiment. After five weeks of training he began his diaries as his regiment, "armed and equipped for Dixie," advanced into Kentucky. In March 1863 Private Coe was captured in Tennessee. "Weary and emaciated," he was sent to Libby Prison in Richmond. A week later he was exchanged, and after seven months of recuperation he returned to limited duty as Union forces gained the upper hand in the Chattanooga area late in 1863.

Corporal Coe was finally healthy enough to return to combat duty as his Nineteenth Michigan Regiment joined Sherman's powerful offensive into northwest Georgia in the spring of 1864. Promoted to sergeant, he saw his Company E cut to pieces in a series of bloody battles before Atlanta fell on September 2. He was again weakened by illness and spent the last months of the war convalescing. Discharged in May 1865, he went home and concluded his last diary, rejoicing to be "once more in God's country and a free man."

Sergeant Coe's diaries and other papers lay for-

gotten in a trunk until his grandson rediscovered them. A combat infantry sergeant in World War II, David Coe was convinced that his grandfather's Civil War diaries merited publication. His experience as a photographer-journalist was helpful, especially with the effective illustrations, but his lack of historical training was a real handicap. The book lacks an index, a bibliography, and footnotes, and the editorial comments are sometimes shallow. Coe appears unaware of the best works of scholarship in recent decades. On the dust jacket the diaries are described as "sometimes nearly illegible," but they are published in flawless English without explanation.

Nevertheless this volume is interesting and informative. Like Bell Irvin Wiley's studies of Johnny Reb and Billy Yank, it shows what the Civil War was like for the ordinary soldier. The historian as well as the general reader can learn from intelligent "little men" like Sergeant Ham Coe.

F. N. BONEY
University of Georgia

BELL IRVIN WILEY. *Confederate Women*. (Contributions in American History, number 38.) Westport, Conn.: Greenwood Press. 1975. Pp. xiv, 204. \$10.95.

Originally presented as a series of lectures, *Confederate Women* consists of four essays: the first three are portraits of Mary Boykin Chestnut, Virginia Tunstall Clay, and Varina Howell Davis; the fourth is a survey of the wartime experiences of Southern women in general. Based on extensive research among letters, diaries, and memoirs of Confederate women, these essays are interesting and occasionally provocative, but they do not significantly enlarge or revise our understanding of Southern women drawn from Anne F. Scott's *The Southern Lady* and Elizabeth Massey's *Bonnet Brigades: American Women and the Civil War*.

Some of the limitations derive from the author's choice of subjects. Although, as Wiley maintains, Chestnut, Clay, and Davis may represent "distinct types of Confederate womanhood: the childless intellectual; the inveterate Southern belle; First Lady, wife and mother," Clay and Davis are notable only because of their husbands' positions, and Wiley's portraits of all three bear little resemblance to the women discussed in the fourth chapter. Moreover, a focus on other types—a woman who managed the family business, or one who provided nursing or other services to the Confederate Army—would more fully illuminate the experiences of elite women during the war. Nor does the author make maximum use of the material on the three primary figures. These chapters are overly anecdotal with a disproportionate emphasis on superficial social relations at the expense

of more profound analysis of how these women perceived and operated within the patriarchal order.

The final essay provides a larger perspective on the war's meaning for women in general. Because Wiley relies on women's letters and diaries and does not employ statistical data, his focus remains on privileged women. Women of the yeoman class are mentioned infrequently, and black women are allotted just five paragraphs. Although his discussion of women's responses to the new demands and hardships brought on by the war is necessarily brief, Wiley mentions a variety of roles—women as factory and government employees, as leaders of bread riots, as petitioners to the government, as wives and mothers of soldiers—which suggest opportunities for further investigation.

SUSAN M. HARTMANN
University of Missouri,
St. Louis

ROBERT P. SWIERENGA, editor. *Beyond the Civil War Synthesis: Political Essays of the Civil War Era*. (Contributions in American History, number 44.) Westport, Conn.: Greenwood Press. 1975. Pp. xx, 348. \$13.50.

This is a useful compilation of the work of a group of the self-styled "new political historians" with their social-scientific orientation. However, not all of the historians in the volume fall into that category. The title is derived from Joel H. Silbey's argument in the opening essay that historians of the middle period of United States history have allowed the Civil War to distort reality by over-emphasizing sectionalism and, in effect, reading backward from the fact of the war to interpret too much of what preceded it. Where others have emphasized rising sectionalism in the 1840s, Silbey's analysis of congressional voting leads him to stress its partisan rather than its sectional nature. In the second essay Eric Foner suggests that some of the new political historians "seem to be in danger of substituting a religious or cultural determinism" to replace an older economic determinism (p. 18). Richard O. Curry's penetrating overview of recent work in the Civil War and Reconstruction period and Stephen B. Oates' survey of the historical literature on John Brown round out part 1, the historiographical portion of the volume.

"Politics at the Grass Roots" is the focus of part 2. David E. Meese analyzes the Northern Democratic party's loss of congressional seats in 1858 and concludes that the development was "as much the result of power, prestige, and patronage as of unpopular Presidential policies about Kansas" (p. 96). Robert P. Swierenga deals with a group of Dutch immigrants in Iowa who stubbornly voted Democratic in 1860, thus defying not only certain

of their leaders but those generalizations about Lincoln's hold on the foreign-born in the Midwest. Lawrence N. Powell argues that the larger issues of Reconstruction had little bearing on the fact that seventeen Republican incumbents were not renominated in 1866, yet in the subsequent essay Michael Les Benedict advances the thesis that in the state and local elections of 1867 in the North, the congressional plan of reconstruction was on trial and was rejected by the voters, thus setting "the limits on reform in reconstruction" (p. 147). Phyllis F. Field studies referenda held in New York on the question of black suffrage in 1846, 1860, and 1869; she demonstrates that while even the Republicans were badly divided on the matter of black suffrage in 1860, they had moved far closer to agreement in support of it by 1869.

Various aspects of roll call voting in Congress are treated in part 3. Although the editor contends in the introduction that "all five of the articles in Part III suggest that party is the most important determinant" (p. xvi), Gerald W. Wolff, employing Guttman scalogram analysis of voting on the Kansas-Nebraska question, insists that both party and sectional influence played major roles but "sectionalism appears to have been most potent" (p. 179). Richard E. Beringer traces an "unconscious" party spirit that developed in the Confederate Congress, and Allen G. Bogue studies bloc and party voting in the Senate from 1861 to 1863 to conclude that since party, geographic bloc, and faction were all important factors, historians should study the "houses of Congress during the Civil War as political systems in which a variety of determinants of voting behavior were interacting" (pp. 222-23). Edward L. Gambill and Glenn M. Linden provide, respectively, more precise categories for the Republican factions in the Senate in 1866 and a roll call analysis of the Senate from 1873 to 1877 which shows that "the Senate had returned to its normal voting habits in the realm of political and economic measures" (p. 241).

The ethnoreligious dimension of politics is the subject of part 4. Leonard Tabachnik argues that, despite the charges of nativists, foreign-born citizens were significantly underrepresented in the Federal Customhouse Service from 1821 to 1861. Richard Jensen uses Midwestern county directories as a basis for his proposition that, outside the South, the pietistic voters (that is, Congregationalists, Methodists, Baptists, and so on) tended to be Republican and the members of liturgical churches (especially Roman Catholic) tended to be Democrats. August Meier marshals traditional, nonquantitative evidence to support his thesis that most of the Northern people in positions of political power during Reconstruction "were not really interested in the Negroes' welfare" (p. 289).

In the fifth and final group of essays, "Ideology and Politics," Larry Gara suggests that a Northern political party was made possible by a "steadily increasing opposition to the slave power" rather than by "any growth of pure antislavery sentiment or humanitarian consideration for the slave as an oppressed human being" (p. 308). Bertram Wyatt-Brown and James B. Stewart deal with William Lloyd Garrison in the last two essays, the first emphasizing that the fiery abolitionist actually helped keep traditional antislavery action in nonviolent channels and the second arguing that the Garrisonian approach to Northern politics in the 1840s and 1850s was neither as unsophisticated nor as unproductive as many historians have assumed.

While the new political historians obviously have much to contribute toward a more precise formulation of various aspects of the history of the middle period, several of the strongest essays are traditional in method and style. Moreover, some of the quantifiers are so preoccupied with their methodology that their findings tend to become subordinated and lost in lumpy prose that is badly scattered among tables. Certain details about the "Civil War synthesis" have been corrected, but there is surely no alternative synthesis yet in sight.

ROBERT F. DURDEN
Duke University

JERRELL H. SHOFNER. *Nor Is It Over Yet: Florida in the Era of Reconstruction, 1863-1877*. (University of Florida Book.) Gainesville: University Presses of Florida. 1974. Pp. x, 412. \$12.50.

Additional sources and a different set of preconceived opinions allow Jerrell H. Shofner to produce a general history of Florida between 1863 and 1877 that is considerably different from William Watson Davis' 1913 Dunning School offering. The new work fits the framework being so gropingly erected by revisionist scholars, and, while not free of discernible blemish caused by modern biases, it ranks with the better efforts of that school currently in vogue. Veering most importantly from Davis in attitude toward black people during the time of Reconstruction, Shofner offers the usual revisionist evaluations, particularly in assessing levels of corruption and crime. Shofner is perhaps justifiably sympathetic in areas where his predecessor was hostile, but the book's most crucial flaw remains that while the author recognizes a program which was clearly opposed by a majority of the local population and which was forced into effect during Reconstruction, he fails to wrestle adequately with this philosophical and weighty matter.

Shofner, a consensus historian, develops the

theses that the period is identifiable neither by sudden nor by substantive historical elements, that in Florida continuity was much greater than change, that conservative Democrats were not much different from the Republicans except in racial attitudes, and that almost everyone's actions were complex and often reciprocally interrelated in the historical context. Shofner fights an ongoing battle against the persistent myth that, in the end, carpetbaggers packed up and headed North and that the leadership change which followed was among the most complete ever.

The work is soundly researched, though some of Davis' sources unfortunately are no longer available. The literary quality is good. The plethora of detail illustrating and supporting the generalizations is occasionally tedious, but with the anecdotes and other data this gives the book some utility as a reference. Commendable chapter summations and a concluding essay render its messages easily discernible. Of special interest are the occasional linkages to national affairs of matters that appeared on the surface peculiarly Floridian.

HERMAN HATTAWAY
University of Missouri,
Kansas City

RICHARD A. BARTLETT. *Nature's Yellowstone*. Albuquerque: University of New Mexico Press. 1974. Pp. xiii, 250. \$10.00.

The 1972 centennial of Yellowstone National Park, the nation's and the world's first, prompted publication of several volumes concerning the region's character and history. They range from the lavish, coffee-table format of Ann and Myron Sutton's *Yellowstone: A Century of the Wilderness Idea* (1972) to Aubrey L. Haines' *Yellowstone National Park: Its Exploration and Establishment* (1974), an exhaustively detailed, documentary approach. Bartlett's book takes a middle course. His treatment is scholarly and well documented yet light in tone. The author clearly loves Yellowstone, and the fact that he enjoyed writing about it is to the reader's decided advantage.

Though not a scientist, Bartlett describes the geological and biological history of the Yellowstone country quite impressively. There are few better places than the opening chapters of this book where one can find out why a Yellowstone geyser erupts or a Yellowstone black bear mooches garbage. Turning to the human history of the region, Bartlett brings together the best knowledge available about its Indian occupants and the first white visitors. The remarkable wilderness wanderings of John Colter are featured in the latter discussion. Bartlett sifts the inconclusive evidence about Colter's actual route and, to his credit as a

historian, frankly confesses that unless new material is discovered, no one can be sure that Colter ever saw Yellowstone between 1806 and 1810.

Another topic, disputed for a century, is the question of whom to credit for inventing the national park idea. In his presentation of the formal exploration of Yellowstone and the establishment of a national park in the region in 1872, Bartlett of necessity enters the argument. His account deals clearly and fairly with the history of the national park concept, although George Catlin's journal proposal of a "nation's park" in the American West came in 1832, not 1833 as Bartlett asserts (p. 191). In his discussion of the candidacy of Thomas Francis Meagher as father of Yellowstone National Park, Bartlett turns new sod. An Irish revolutionary who escaped from exile in Tasmania to become governor of the Montana Territory, Meagher received reports of Yellowstone's wonders from trappers, miners, and a Jesuit priest to the Blackfeet, Francis Xavier Kuppens. Meagher's 1865 suggestion—if correctly reported by Father Kuppens—that the Yellowstone region be reserved "for a national park" is not only the first specific suggestion of a park in the area but the first known use of the phrase "national park" in American letters. Had not Meagher drowned in the upper Missouri River in 1867, he might well have implemented his plans to explore Yellowstone and advocate park status.

If the present volume may be taken as evidence, Bartlett's proposed continuation of Yellowstone's history from 1872 through the park's uncertain early years should be eagerly awaited.

RODERICK NASH
University of California,
Santa Barbara

LEONARD J. ARRINGTON. *Charles C. Rich: Mormon General and Western Frontiersman*. (Studies in Mormon History, volume 1.) Provo, Utah: Brigham Young University Press. 1974. Pp. xvii, 386. \$7.50.

The significant theological and cultural units in Mormonism are the family and the church; individuals matter less. Perhaps that explains why biography, a congenial genre for most American authors, is a difficult form for Mormon scholars. In biographies written by Latter-day Saints the individual is so often overshadowed by his world that most Mormon biography is really family history or, more likely, church history that merely highlights the activities of the biographer's subject. Leonard J. Arrington's biography of Charles C. Rich, an apostle whose importance rests mainly on his military leadership and his colonization efforts in San Bernadino and Bear Lake Valley, is no exception. Rich's story dominates the early chap-

ters, but when the history of his life as Latter-day Saint begins, the emphasis shifts. At times the background obscures the foreground to such an extent that the events in Rich's life seem almost incidental. While information is admittedly sparse, the problem is less a silent record than misplaced focus. Intended as Rich's story, the study intermittently assumes the character of a narrative history of Mormonism. Nevertheless, this is not a run-of-the-mill Mormon biography. Conjecture is identified with scrupulous care; quoted material is reproduced—as a Rich descendant reminded me—with misspellings intact; and pious platitudes are generally absent. Despite its shortcomings, historians will find this a better biography than the 1936 panegyric it replaces.

A scarcity of scholarly studies of the leadership is a serious lacuna in Mormon history. Acceptable biographies of Brigham Young, John Taylor, George Q. Cannon, and other major figures do not exist. Since Arrington's knowledge of the Mormon past is second to none, since his respect for the canons of historical scholarship is manifest, and since his access to the sources is virtually unlimited, historians can only regret that he chose to study a Latter-day Saint leader of the second rank.

JAN SHIPPS
*Indiana University—Purdue University,
Indianapolis*

JOHN M. CARROLL, compiled, edited, and written by. *Custer in Texas: An Interrupted Narrative*. New York: Sol Lewis and Liveright. 1975. Pp. xx, 288. \$15.00.

ALBAN W. HOOPES. *The Road to the Little Big Horn—And Beyond*. New York: Vantage Press. 1975. Pp. 336. \$9.50.

BRUCE A. ROSENBERG. *Custer and the Epic of Defeat*. University Park: Pennsylvania State University Press. 1974. Pp. xiii, 313. \$13.50.

Here are three of the many books on George Armstrong Custer that are appearing during the centennial year of the Battle of the Little Bighorn. Beyond involving Custer in one way or another, the three have little in common.

The one of broadest appeal is Bruce A. Rosenberg's *Custer and the Epic of Defeat*. It is literary analysis rather than history. Indeed, as history it is deficient, for the author's knowledge of the history of the Battle of the Little Bighorn is superficial and sometimes in error, and it rests on a very small sampling of the extensive bibliography of the subject. As literary criticism the book will be received as a penetrating study of the process of legend-making. Taking Custer and his "last stand" as the model, Rosenberg shows that certain enduring

principles shape legends about heroic figures and that they can be discerned in the literature of such diverse characters as Saul, Roland, Leonidas, and others over the past twenty-five centuries. Although occasionally murky and irrelevant, the comparisons for the most part are illuminating and often fascinating.

Alban W. Hoopes is remembered by students of Indian history chiefly for a slim but useful volume entitled *Indian Affairs and Their Administration, with Special Reference to the Far West, 1849-1860*, originally a doctoral dissertation. With *The Road to the Little Big Horn—And Beyond*, he returns to publish in this field after an absence of more than forty years. This is not simply a history of the Battle of the Little Bighorn but rather of relations with the Indians of the northern Plains from Sand Creek in 1864 to the surrender of Sitting Bull in 1881. It draws on an unusually broad bibliography, including private papers and official records. Even so, it adds little to an oft-told tale and is in places uneven.

The final volume will be of interest mainly to Custer specialists. Handsomely designed and printed and illustrated with original artwork. *Custer in Texas* deals with the eight months following the close of the Civil War during which time the twenty-five-year-old general was commander of a mutinous cavalry division sent to Texas for possible use against Maximilian's forces in Mexico. This is billed as an "interrupted narrative"—it is Elizabeth Custer's account, taken from her *Tenting on the Plains*, interrupted by editor John M. Carroll for his own remarks or for the insertion of material from other sources.

ROBERT M. UTLEY
National Park Service

BINGHAM DUNCAN. *Whitelaw Reid: Journalist, Politician, Diplomat*. Athens: University of Georgia Press. 1975. Pp. 305. \$11.00.

Whitelaw Reid (1837-1912) owned and edited one of the great newspapers of his time, the *New York Tribune*; was an important political figure for more than a quarter of a century, running for vice-president on the Republican ticket in 1892; and served as minister to France, as a delegate to the peace conference after the Spanish-American War, and as ambassador to Great Britain.

Surely a man of this importance deserves to be studied. Yet the only complete biography of Reid previously published was written by Royal Cortissoz, a good friend and ardent admirer, more than fifty years ago. Bingham Duncan amply fills this lacuna with this slender and excellent volume. Using a variety of manuscripts and standard works,

Duncan has written a solid and absorbing new account of Reid's public life.

Duncan does not try "to support or destroy any preconceived theory of Reid's significance nor to make him seem to be a force for progress or a negative influence on developments in his period." Within this framework he achieves his purpose. Though his study will not entirely supplant the Cortisoz volumes, it probably will become a standard work, and it surely makes a significant contribution to American society and politics during the Gilded Age.

Duncan shows that Reid was a supporter of civil service legislation, sound money, and a protective tariff in politics. In foreign affairs he opposed any entanglement in European affairs, supported the Monroe Doctrine, believed in American superiority in the Caribbean, and eventually became an outright imperialist advocating a course of colonialism both in the Philippines and the Caribbean. While he often mentioned duty in discussing colonialism, his arguments were mainly deterministic, putting him with the Social Darwinists.

This is a balanced, readable, and well-researched book, and it should appeal to scholars and general readers alike. But I wish the author and publisher had put the footnotes at the bottom of the pages where they belong.

VINCENT P. DE SANTIS
University of Notre Dame

and significance of immigrant mobility. Thus he surmises that in South Bend "the immigrant might hope to find a community whose social and economic structure was not already fixed"; that such segregation as existed "might have been based" on ethnic preferences; or that a marginal difference in literacy somehow "helps to explain" a group's success.

Some inferences seem contradictory: from the general rate of success Esslinger concludes that South Bend must have attracted immigrants with "more money and ambition" straight from the dock—or conceivably they had gained useful experience during lengthy stays along the way—and yet lack of success among them is held to imply previous failure in the East as well. Some conclusions are tautologies: Germans score higher than Englishmen (11–9) as leaders of the general urban community mainly because Esslinger counts officeholding in ethnic societies (six of them German, none English) among his indexes of public leadership, without ever demonstrating how such societies involved either "daily contact" among ethnic groups or "adjustment to an American way of life." If the remaining two hundred-odd cities of the time are in line for such studies, the analysis will have to be considerably less tenuous than this.

ROWLAND BERTHOFF
Washington University

DEAN R. ESSLINGER. *Immigrants and the City: Ethnicity and Mobility in a Nineteenth-Century Midwestern Community*. (Interdisciplinary Urban Series.) Port Washington, N.Y.: Kennikat Press. 1975. Pp. xii, 156. \$9.95.

To the half-dozen American cities of 1850–80 that have been quantitatively analyzed since 1964 on the lines of Stephan Thernstrom's Newburyport model, Dean R. Esslinger now adds South Bend.

This Midwestern city displayed some differences. Although the Irish, French Canadians, and Poles of South Bend were residentially more concentrated than other ethnic groups, no ward had more than a fraction of any group; this resulted from the tendency of newly arrived immigrants to settle near recently established local industries. Except for the Irish, immigrants who "persisted" in town for more than a decade (as elsewhere, most did not) enjoyed far greater upward mobility to skilled and white-collar jobs than in Newburyport or Boston, and their sons continued the climb.

Such measurements are useful enough. Since the book lacks the qualitative half of the Thernstrom model, however, Esslinger often resorts to speculation—or to deduction from propositions of Thernstrom's and Handlin's—to explain the causes

JAMES M. YOUNGDALE. *Populism: A Psychohistorical Perspective*. Port Washington, N.Y.: Kennikat Press. 1975. Pp. 220. \$15.00.

James Youngdale, a Minnesota farmer and laborite-turned-academic, has two aims in this book. The first is to rescue Populism from historians like Richard Hofstadter who have confused its radical social programs with the hysteria of mainstream "Bryanism." Both movements developed as the nineteenth-century small-producer paradise shattered. William Jennings Bryan, his followers, and his successors, trapped inside a self-made dream, invented monsters to explain the loss of their world. Populism was not free of Bryanist elements, but because it grasped the social character of existence, it could explain social change without demonology and offer concrete programs of community control. Under the influence of labor and socialist elements, which played a larger role in grass-roots Populism than traditional historians have admitted, Populism moved in a "radical neomercantilist" direction.

Youngdale wants psychology, like history, to center on the social world. Thus, his second aim is to replace a Freudian interpretation of mass movements with an Adlerian one. While Freud empha-

sized inner anxiety and unconscious needs, Adler rejected "internalized personality structure" (p. 58) for a psychology in which "the attitude toward others" (p. 61) generates goal-oriented strategies for making sense of the world. Adler makes social movements rational and offers, against Freudian and American individualism, an ideal of human cooperation.

The book's opening chapters are wide ranging, impressive, and unstereotyped. The actual studies of Populism offer valuable vignettes, particularly for twentieth-century Minnesota. If the presentation is episodic, Populism's relation to Bryanism is still a major theme. Adler, however, completely disappears. Although a psychology that does away with interior life flattens out experience, it may have insights to offer. But Adlerian psychology, whatever its qualities, does not inform the actual history written here. Youngdale uses Adler to refute Hofstadter's Freudianism, not to ground a psychological reading of his own. *Populism: A Psychohistorical Perspective* has a strong purchase on Populism. It is not psychohistory.

MICHAEL ROGIN
University of California,
Berkeley

JEFFREY G. WILLIAMSON. *Late Nineteenth-Century American Development: A General Equilibrium History*. New York: Cambridge University Press. 1975. Pp. x, 350. \$19.50.

Economic historians have dealt with nearly every aspect of American development during the years between the Civil War and World War I. Jeffrey Williamson believes, however, that our knowledge and understanding is still lacking because scholars have not used proper research methods to answer the key questions connected with economic development. To deal with what he considers the shortcomings of previous researchers, Williamson uses the techniques of model building, counterfactuals, and general equilibrium analysis. This volume, therefore, is filled with equations, graphs, charts, and statistical tables that will deter all but the most determined reader.

In chapter 3 the author develops a long and detailed model for regional growth, and he argues that the model accurately simulates actual economic development between 1860 and 1910. He then applies this model to six major issues: secular decline after 1870, the national capital market, agricultural discontent, the role of railroads, overseas trade, and immigration. By dealing with these issues in "actual" terms and then applying his counterfactual history to the same events, Williamson comes up with some interesting conclusions.

Some of his positions are new and even startling,

although questionable. Williamson concludes, for example, that the economy performed inefficiently from the late 1860s to the early 1870s because too few resources were devoted to agriculture. He also argues that land availability had very little effect on economic growth after the Civil War, and that the supply of relatively cheap land had no significant effect on immigration to the United States. On the other hand, the author simply substantiates what most scholars have long accepted. Such conclusions as "world market conditions had a profound effect on the agriculture of the Midwest" (p. 213), or "An America without immigrants indeed would have grown very differently from how in fact she did in the late nineteenth century" (p. 249), hardly seem worth the author's complicated methods.

The evidence is not yet all in on the development of the American economy between 1860 and 1910. This volume, however, provides more speculation than understanding. There is little doubt that the book will arouse controversy and debate, but it is more distinctive in its methodology than in what it shows us about the developing economy of the United States between 1860 and 1910.

GILBERT C. FITE
Eastern Illinois University

ALBERT W. NIEMI, JR. *State and Regional Patterns in American Manufacturing, 1860-1900*. (Contributions in Economics and Economic History, number 10.) Westport, Conn.: Greenwood Press. 1974. Pp. xiii, 209. \$11.00.

FREDERICK MOORE BINDER. *Coal Age Empire: Pennsylvania Coal and Its Utilization to 1860*. Harrisburg: Pennsylvania Historical and Museum Commission. 1974. Pp. 184. \$5.50.

Neither of these studies significantly alters traditional views of the nineteenth-century American economy. The more ambitious and successful of the two books is Albert W. Niemi, Jr.'s treatment of changing state and regional patterns in manufacturing. Niemi's study is based on data from the 1860 and 1900 published federal manufacturing censuses. In order to compare more systematically and accurately information from the two censuses, Niemi performed the arduous task of classifying all the data into twenty major manufacturing groups according to the Standard Industrial Classification (SIC) two-digit industry categories. The tables throughout the text and an appendix of nearly one hundred pages present—in SIC categories by state and region—estimates of value added, employment, labor costs, capital invested, output-labor ratios, capital-output ratios, and capital-labor ratios for 1860 and 1900. This presentation of data in

a usable and comparable form is Niemi's major contribution.

In the brief text that accompanies the tables, Niemi compares the structural and locational pattern of manufacturing in 1860 with that of 1900. He finds that manufacturing was concentrated in the Northeast in 1860 and that there was a gradual drift of industry into the Midwest and South by 1900. These changing locational patterns, he concludes, depended less on supplies of capital and labor than on resource conditions, local demand, and external economies. Niemi's conclusions confirm earlier views, although no previous work rested on this revised data base. There is little reason to doubt the author's general conclusions, but questions and opportunities for further study remain. Analysis of the manuscript rather than the published census, which is now underway, may allow for alternative or better answers to these questions, especially in providing causes for the structural shifts in manufacturing. It may also be possible—it is certainly desirable—to replace census data defined by state boundaries with data that reflect more accurately the market realities of economic geography. And, while Niemi briefly sketches manufacturing changes after 1900, more detailed comparisons with early twentieth-century structural and locational changes are needed. As a reference source and as an introduction to late nineteenth-century state and regional patterns of manufacturing Niemi's book is a useful addition to the literature.

Frederick Moore Binder's theme in *Coal Age Empire* is the use of Pennsylvania coal, bituminous and anthracite, before 1860. He concentrates on developments in home heating, urban gas lighting, steamboats, locomotives, and fuel for industrial growth. The book is weakest in consideration of transportation and industrialization—areas where Pennsylvania coal most affected the course of antebellum economic development. Binder does not relate systematically increased use of coal to major changes in technology, markets, or the industrial order. He neglects to analyze fully and explicitly such fundamental factors in the West as the correlation between use of coke, exploitation of the Connellsville coal region, and the market for rails or, in the East, the timing and nature of transportation access to anthracite fields. Knowledge of river, canal, and railroad transportation from the Pennsylvania coal regions is a necessary prelude to understanding the use of coal and its relationship to industrial change, but Binder largely ignores this subject until the final chapter, a shortcoming that illustrates the book's poor organization and inadequate attention to major questions.

Although the author presents interesting and useful facts, scholars studying the significance of

coal in the antebellum economy will also wish to read Peter Temin's *Iron and Steel in Nineteenth-Century America* (1964) and Alfred D. Chandler's essay on anthracite coal in the *Business History Review* (summer, 1972), neither of which is cited in Binder's footnotes or bibliography.

JAMES H. MADISON
Indiana University,
Bloomington

JAMES GILBERT. *Designing the Industrial State: The Intellectual Pursuit of Collectivism in America, 1880-1940*. Chicago: Quadrangle Books. 1972. Pp. ix, 335. \$10.00.

James Gilbert believes that too many American liberal intellectuals have been mesmerized by stereotypical political terms despite the mountain of revisionist writing on progressivism and socialism. He believes there is a language of politics that often evades reality in order to defend clichés. The clichés are, of course, words like liberal, conservative, progressive, socialist—the parlance of both progressive and revisionist history. Gilbert's use of the concept of collectivism as an umbrella for the thought and activities of Americans who span the whole political spectrum is extremely effective and makes intellectual sense out of what was before a series of anomalies.

The book seems to grow naturally out of the W. A. Williams Wisconsin school that called into question much of progressive and liberal-consensus history. But Gilbert is not trying to provide yet another explanation for the failure of socialism or the triumph of liberal-capitalism. Rather, he sets out to demonstrate the almost apolitical impact of post-Civil War corporate reorganization of American social-economic life. In the course of his discussion he encompasses such familiar themes as "response to industrialism," "triumph of conservatism," "search for order"; but his essential thesis is that in the welter of ideas—from socialism to liberal-conservative corporatism—there is a common source. That source is experience with and observation of the developing forms of the American corporation. Thus the intellectuals who struggled to extract, or impose, meaning and purpose had as their connecting link an appreciation of the need to accept or mold collectivism. They might argue about the exact role of the state, but not really about the inevitability of collectivism. The line from an Edward Bellamy or a Laurence Gronlund to a James Burnham or an A. A. Berle becomes clear.

Gilbert shows us why American political categories have been blurred, why Americans have gone back and forth across "political" lines, and why varieties of collectivist thinking have been

much more important than the traditional gloss of liberalism in the shaping of contemporary America. His discussion of inevitability and of the role of intellectuals, if occasionally contentious, is always illuminating. Perhaps Gilbert's most important contribution is increasing our understanding of the indigenous sources of American social-political thought that, ironically, improves considerably the comparative perspective in American history.

KENNETH MCNAUGHT
University of Toronto

THOMAS R. COX. *Mills and Markets: A History of the Pacific Coast Lumber Industry to 1900*. (The Emil and Kathleen Sick Lecture-Book Series in Western History and Biography.) Seattle: University of Washington Press. 1975. Pp. xx, 332. \$17.50.

CHARLES E. TWINING. *Downriver: Orrin H. Ingram and the Empire Lumber Company*. Madison: The State Historical Society of Wisconsin. 1975. Pp. ix, 309. \$17.50.

These two volumes are studies of the late nineteenth-century lumber industry, and both are expansions of dissertations. Thomas R. Cox examines the companies of the littoral areas of the three contiguous West Coast states and British Columbia. Charles E. Twining confines himself to one firm in Eau Claire, Wisconsin, emphasizing its downriver marketing activities. The works are of high quality—Cox received the Emil and Kathleen Sick Award from the University of Washington—but both have shortcomings.

A major fault in Twining's study is the scant attention given to sources other than Empire Lumber Company papers. There are only three references to other manuscript collections. I waited in vain for some comparisons between the Empire Lumber Company and its neighbor, the Daniel Shaw Lumber Company. This narrow focus makes it difficult to evaluate Ingram's policies or to understand the company in its historical context. Only one of Ingram's contemporaries, Frederick Weyerhaeuser, is considered in the study. I have not visited Wisconsin county courthouses, but, judging from experience in other states and from the descriptions in WPA inventories of Wisconsin county resources, many relevant documents were not used.

Cox's purpose is to "provide insights . . . into the forces . . . forging the modern, industrial United States" (p. xii). With this goal, the slight treatment given the subject of land is difficult to defend. If technological changes warrant a chapter, the problems of resource access that forced many of these innovations are worthy of greater consideration. On the same point, the extent and location of lumber company land holdings exerted

an influence on regional development and remain a significant economic and social factor; this is a major omission. I am also dissatisfied with the abbreviated treatment of labor. Cox notes that expanded coverage would have meant writing a different book (p. xii). But an understanding of the industry notorious for labor strife is impossible without an explanation of its attitudes and policies toward labor.

Cox is at his weakest on the details of the industry. For example, the first logging railroad was put into operation in the Puget Sound Basin in 1876, not in 1881 (p. 218), George A. Meigs did not restore the Port Madison mill to solvency in the late 1870s (p. 109), and sawdust was blown into furnaces to increase efficiency, not just to speed disposal (p. 237).

Neither author uses econometric methods, nor does either fully use the available statistical evidence. Twining lists without comment Corps of Engineers figures for log and lumber movements on the Chippewa River. If the figures are incorrect, their inaccuracy should be noted; if they are reliable, they should be used more fully. Cox views available statistics with skepticism but fails to seek alternatives. The House Report on Internal Commerce during the Fifty-first Congress would have been helpful in evaluating these data. Cox apparently did not consider substituting shipping tonnage for board feet as a more accurate indicator of volume.

These complaints aside, both volumes are valuable additions to the literature on the lumber industry. Twining has contributed a rare view of the internal workings of a frontier enterprise. Cox has sketched the broad outlines of important aspects of the West Coast industry in a manner that will help scholars place localized studies in proper context.

THOMAS F. GEDOSCH
The National Archives

CHARLES PIERCE LEWARNE. *Utopias on Puget Sound, 1885-1915*. Seattle: University of Washington Press. 1975. Pp. xiv, 325. \$12.50.

In this book Charles Pierce LeWarne traces the rise and fall of five communitarian colonies located in western Washington during the Populist-Progressive era. These experiments were the Puget Sound Co-operative Colony, Equality, Freeland, Burley, and Home. Although they all exemplified the revival of utopianism at the end of the nineteenth century, each venture had varied initial objectives. Puget Sound Co-operative members desired communal living; Equality's founders wanted to "capture" Washington state for socialism; Freeland residents sought cooperative merchandizing based on semicapitalistic principles;

Burleyites dreamed of the "Co-operative Brotherhood"; and Home colonists hoped to create a land-holding institution where individuals with unorthodox views might live in peace.

Paralleling other contemporary colonies, these Washington state utopias were secular in nature and practiced some form of economic cooperation. And like the others, they proved ephemeral. The reasons for their demise fit a common pattern. Internal strife, which decimated so many experiments throughout the annals of American utopianism, struck the Puget Sound colonies. They also, according to LeWarne, "failed to create the sense of commitment that might have spelled greater success or endurance."

This detailed work is a fascinating study of obscure utopias. The overall scholarship is superb, the style crisp. The book, however, is weakened slightly by LeWarne's scanty treatment of why utopian colonies arose at this particular time. He notes in his conclusion that "the rise of communitarianism in the 1890s was in part a response to the severe depression," yet he never fully explores this apparently valid interpretation. There is another flaw. Although LeWarne attempts to trace the interrelationships between the Puget Sound utopias and other contemporary colonies, he might have examined more closely the reform-utopia press. Papers like the *Altruist*, *Integral Co-operator*, *Progressive Thought and Dawn of Equity*, *Co-operative Reform*, and *Co-operation* reveal the often close connection between the various late nineteenth-century utopias. For example, several members of the Freedom Colony in Kansas moved to Burley, while some residents of the Puget Sound Co-operative Colony joined the Colorado Co-operative Company. Nevertheless, LeWarne has produced a major contribution to the understanding of a largely neglected phase in the history of American utopianism.

H. ROGER GRANT
University of Akron

KENNETH M. ROEMER. *The Obsolete Necessity: America in Utopian Writings, 1888-1900*. Kent, Ohio: Kent State University Press. 1976. Pp. xiv, 239. \$10.00.

Since the decline of the myth-symbol school, there has been no widely accepted approach to the problematical relationship of literature and culture within American studies. Kenneth Roemer's examination of utopian writings is part of a recent attempt within the discipline to formulate a new approach to the problem. He acknowledges that high culture does not necessarily reflect popular attitudes. He also recognizes the difficulties of assuming that popular literature mirrors cultural attitudes. Roemer's methodological solution is a

"multilevel" analysis of utopian writings. Examining the average, the popular, and "the best" works, he argues that if they shared some values, these values were reliable indexes of popular attitudes in two main ways. First, what authors kept in their utopias pointed to the values that middle-class Americans most cherished, a homogeneous population and traditional sex roles, to name only two. Second, when utopian writings at all levels showed ambivalence toward change, this indicated ambivalence within the culture. Utopian works typically desired change while valuing stability and order. This ambivalence permeated their treatment of many topics, especially the city.

Such a schematic summary cannot do justice to Roemer's ability to relate this paradox to the social context. Nor can it explain the way in which he links utopian writings to the problems that their middle-class WASP authors were coping with. Neither the larger context nor the writers' intentions are slighted in favor of literary analysis. Roemer does not always clarify, however, whether utopian works mirrored the values of the group to which the writers belonged or those of Americans generally. He seems uncertain whether the culture was unified enough to permit the latter to be true. This leaves open the question of how a culture should be conceived. Many people in American studies have abandoned the holistic view, and one wishes that the author had been less ambiguous about this issue. Aside from this, Roemer presents a method for relating literature and society, ideas and culture, that has wide applicability.

JEAN B. QUANDT

The Institute for Research in History

ROBERT HESSEN. *Steel Titan: The Life of Charles M. Schwab*. New York: Oxford University Press. 1975. Pp. xvi, 350. \$14.95.

Robert Hessen has written a strong, affirmative study of the business career of Charles M. Schwab. He portrays Schwab, a leader in the American steel industry from the 1890s to the 1930s, as an outstanding example of what a man of drive and ingenuity could accomplish in a free enterprise society. The best portions of the biography coincide with the creative phases of the steel magnate's life. The author deftly traces his swift rise to pre-eminence in Carnegie's empire; Schwab's skill in restoring full production at Homestead following the Great Strike of 1892 marked a high point in his career and in the book. Yet Schwab's resourcefulness in helping form U.S. Steel in 1901 was not matched by his ability to seize and hold control. In surrendering the presidency of that firm in 1903 he suffered the signal defeat of his career. The second half of the biography centers on Schwab's restruc-

turing of Bethlehem Steel and elevating it to second place in the industry. Here Hessen makes a convincing case for Schwab's talents as an entrepreneur.

Some of Hessen's interpretations will raise objections. Schwab was repeatedly involved in questionable business ventures—the armor plate scandal, the shipbuilding scandal, and the Bethlehem executives' bonus plan, to name but three. For each Hessen sharply slaps Schwab's wrists. He chides Schwab for lacking candor and for defending too feebly his property rights against meddling officials. In the end, Hessen declares, the cases against Schwab were "not proven." From the author's straightforward presentation of evidence on both sides, readers will be able to reach their own conclusions.

In the area of labor relations Hessen objects to charges that Schwab exploited his workers. "He was the one indispensable man . . . He was the entrepreneur, the *Arbeitgeber*—the work-giver, the man who created jobs" (p. 198). The author apparently sees nothing inconsistent in praising Schwab as a bold innovator when he departs from industry-wide practice in quest of profits, while excusing him for slavishly adhering to the industry's low standards for labor. The author similarly accepts Schwab's arguments—at different times—for paying enormous bonuses to company executives and for cutting tonnage rates for workers as "incentives" to greater production.

These views to one side, Hessen has produced an excellent book based on extensive research in public and private archives, including those of Bethlehem Steel. The biography is informative, insightful, and a pleasure to read.

GERALD G. EGGERT
Pennsylvania State University

LOUIS R. HARLAN *et al.*, editors. *The Booker T. Washington Papers*. Volume 4, 1895–98. Urbana: University of Illinois Press. 1975. Pp. xxx, 593. \$17.50.

This fourth volume of the Booker T. Washington Papers, edited by Louis R. Harlan and his associates, contains 491 items, including correspondence, speeches, newspaper and magazine articles, a diary excerpt, a grocery receipt, and a student petition. The documents range in length from a single-line telegram announcing the death of Washington's stepfather on January 29, 1896, to an eight-page discussion of industrial education by Washington which appeared in the *Independent* two years later. The volume opens with James Creelman's New York *World* account of the Atlanta address of 1895 which catapulted Washington into national prominence and ends with a letter from

Collis P. Huntington in December 1898 promising substantial gifts to Tuskegee Institute. The recognition accorded Washington during these years, including an honorary degree from Harvard and a visit to his school by President William McKinley, signaled his emergence as the pre-eminent black man in America. This volume provides rich insight into the multiple roles he played in his rise to power.

A majority of the documents relate to Tuskegee Institute which Washington still considered the principal base of his operations. With the aid of Northern white millionaires he continued to expand the faculty, student body, and physical facilities of the school. Deference may have characterized his relations with whites, but Washington reigned as a despot on the Tuskegee campus where his "will and policy" were "carried out in the remotest corner of the school" (p. 145). He scrutinized every detail of the institution's operation and promptly disciplined faculty and students who violated his prescribed code. The documents relating to the dismissals of Edward H. Wilson and William Jenkins, two faculty members charged with improper conduct toward female students, underscore both the extent of Washington's authority and the zeal with which he protected the reputation of the school.

Since Washington increasingly became involved with black concerns beyond Tuskegee in the years 1895–98, the papers in this volume represent, as the editor suggests here, "an odyssey of the colorline." With the aid of T. Thomas Fortune, a New York journalist, and Emmett J. Scott, a Texas editor who became his secretary in 1897, he acquired strategic allies in black communities throughout the nation. Always conscious of his "peculiar" position, Washington avoided too close identification with black organizations and utilized conservative language even in protesting the disfranchisement of Louisiana blacks. Only once, in 1898 at the Chicago Peace Jubilee, did he attempt to go beyond the race-relations formula of his Atlanta speech, but the hostile criticism from the South prompted a hasty retreat.

As with previous volumes, this one is a model of imaginative and meticulous editing. The extensive notes not only serve the customary purposes of identification and explanation but also dissect myths such as those surrounding George Washington Carver. Clearly the editor has adhered to his original aim of utilizing the Washington Papers to reveal the complexity of American race relations and the richness of black culture in the late nineteenth and early twentieth centuries.

WILLARD B. GATEWOOD, JR.
*University of Arkansas,
Fayetteville*

GERALD F. LINDERMAN. *The Mirror of War: American Society and the Spanish-American War*. Ann Arbor: University of Michigan Press. 1974. Pp. viii, 227. \$10.00.

The Mirror of War merits more than this short review. It displays a great deal of research, though the author sometimes relies too heavily on such doubtfully representative sources as Carl Sandburg's and Sherwood Anderson's war memoirs to advance his ambitious thesis. Gerald Linderman usually writes gracefully; he also manages at times to produce prose as hard to grasp as shaving cream and phrases such as "pinched in pride and pocketbook by these processes" (p. 4).

But what does his book say about American society during the Spanish-American War? Following up some of Robert Wiebe's perceptive arguments, Linderman proposes that the United States in 1898 was in the throes of fundamental transformation: that "localities" and old-fashioned moral precepts were giving way to a dominant "center" (Washington) and more complex and sophisticated ideas; that Americans generally strove to retain the pre-industrial "old consensus" in the crusade against Spain; but that they ultimately managed only to delay the full ascendancy of the new order.

Through a set of loosely connected essays, Linderman puts forward this thesis in his individual studies of Senator Redfield Proctor's electrifying speech of mid-March 1898, McKinley's decision to go to war, the relationship between small-town communities and their own hometown soldiers, the meaning of the war to individual combatants, American perceptions of their Spanish enemies and Cuban "allies," and the role of the popular press in bringing about and reporting the war.

By no means flawless or without distortions, *The Mirror of War* commands one to look anew at how Americans saw themselves and others three generations ago.

ROBERT L. BEISNER
American University

WILLIAM M. ARMSTRONG, editor. *The Gilded Age Letters of E. L. Godkin*. Albany: State University of New York Press. 1974. Pp. xxiv, 383. \$30.00.

ARTHUR A. EKIRCH, JR. *Progressivism in America: A Study of the Era from Theodore Roosevelt to Woodrow Wilson*. New York: New Viewpoints. 1974. Pp. x, 308. Cloth \$10.00, paper \$4.95.

WILLIAM L. O'NEILL. *The Progressive Years: America Comes of Age*. New York: Dodd, Mead & Company. 1975. Pp. xi, 166.

Edwin L. Godkin, the pungent libertarian and "reform" editor of the *Nation* and the New York

Post, worried from time to time that a democracy too impatient to solve social problems might mortgage its future to politicians, party government, and state power. At the end of his life, indeed, a national enthusiasm for strong government to achieve Progressive reforms had begun to transform the mild liberalism that Godkin had so vigorously defended. William O'Neill and Arthur Ekirch address themselves to this modernizing transformation, and though both historians share the current conventional judgment that Progressives were needlessly cautious and compromising in the use of political power for social ends, they depict the resulting decline of the liberal ideal, paradoxically, in terms that may remind readers in the libertarian 1970s of the dark forebodings of a Gilded Age editor nearly a century before.

Armstrong offers a comprehensive edition of the several hundred surviving, private letters that Godkin wrote between 1864 and his death in 1902. Woven into the text are succinct annotations of biographical comment that make this the first reliable account of the fearless, self-righteous editor's complex relations with his staff, his backers, and his circle of elite friends. With this book and his previous one, Armstrong has established himself as Godkin's only scholarly biographer, though his insights do not match those of Geoffrey Blodgett's recent penetrating essay on Mugwump politics, and the reader who would best understand Godkin's place in American letters should turn also to Morton Keller's 1966 reissue and brief, balanced assessment of Godkin's principal essays on democracy.

Both O'Neill and Ekirch offer a compressed synthesis of established scholarship as a way of fastening a unifying theme on the Progressive era. At first sight O'Neill's appears the more promising. He makes use of more recent findings, particularly on the origins of reform and its actual results in politics, culture, and the economy. His prose is sardonic, witty, and casual, and it enhances a rather simple theme: Progressives aimed sometimes at reform but principally at modernization, and despite a degree of commitment to the general welfare, their programs benefited mainly the middle and upper classes. Those familiar with the major works of Hays, Wiebe, Thelen, Hofstadter, Mowry, Link, and Blum, whose findings O'Neill openly acknowledges and summarizes, will find nothing here to startle them. His observations on foreign policy, however, succumb to clichés; social thought he treats with surprising superficiality; the European context he mentions not at all; and of the many strands of history that he singles out for comment, only regulation, resource management, political reform, and wartime mobilization does he successfully connect to his theme.

Ekirch's version of the story is not always convincing but is more original and provocative. It pictures the fulfillment and decline of a liberal society; by the 1900s, in response to accelerated economic growth and social change, liberals everywhere came to rest their hopes for progress on the national state. Americans were no exception. Many had thought that their palpable edge in abundant resources, their geographical security, and their diffused political structure would permit them to escape centralization of power or at least soften its effects. It was not to be. The forces that were reshaping the liberal societies in Europe engulfed the United States with equal intensity and were unmistakably erasing the distinctions among them. By 1914 national progressivism had triumphed over the older liberal ideal of a decentralized power.

Ekirch is not very clear about the nature of the relationship between the liberal and nationalistic impulses that are critical to his study. He permits one to infer that nationalism was a product of the very forces that gave life to liberalism—a rational drive for social control in an age of profound social disturbance—and that nationalism alone made it possible for liberalism to survive in the modern era. This was a view that Godkin shared with his contemporaries in the Civil War era. But there is also in Ekirch's account a hint of an alternative relationship, and a more fateful one: nationalism gave form to atavistic impulses rooted in historic human behavior; against those impulses the ideals of liberals, including the ideal of international peace, could make no headway. Ekirch unfortunately does not explore either relationship and renders instead a less-complex judgment: the failure of Progressives to fulfill liberal ideals lay in their unwillingness to set firm, precise, and effective limits to national power both at home and in the world. This unwillingness, he would have us believe, made it fatally easy for the United States to accept war as an instrument of national policy. In conclusion, it is possible that the nation could not have done otherwise, but again perhaps it could have; and what a pity it did not try.

Ekirch's judgment is ironic for it invokes the very "exceptionalism" for the American people that he has been at such pains to demonstrate was never within their power to sustain. That was precisely the liberals' dilemma in denouncing American imperialism in the 1890s as a betrayal of democratic ideals. Neither Ekirch nor O'Neill finds much to admire in the political and social prejudices of Godkin's Mugwump readers, and in this they speak not only for their own times but for the Progressive generation itself. It is nevertheless instructive to note in the work of two modern historians echoes of the contradictions in the thinking of

an old liberal reformer concerning America's historical uniqueness and, in consequence, more than a trace of his moral indignation at the road not taken.

OTIS A. PEASE
University of Washington

ROBERT SHERMAN LA FORTE. *Leaders of Reform: Progressive Republicans in Kansas, 1900-1916*. Lawrence: University Press of Kansas. 1974. Pp. vii, 320. \$11.00.

This valuable study of the emergence and performance of Progressive Republican leaders in Kansas adds to the political history of that state and enriches our knowledge of progressivism on the national scene. In the arresting first chapter the author characterizes the nature of the movement in Kansas, compares it with its counterpart in other states, and shows its relationship to the earlier populist impulse. La Forte shows a thorough grasp of his subject and presents it in a restrained, persuasive, direct manner.

La Forte points out that rising economic expectations on the part of the smaller economic interests desirous of a larger share of the growing profit opportunities certainly contributed notably, but not exclusively, to the growth of the Progressive movement in Kansas. He writes that "just as important in creating unrest were party factionalism, ambition for office, and a sincere interest in furthering democratic idealism." He also calls attention to differences in the origins of progressivism. In California and Ohio it began in urban centers and moved upward to the state level, but in Kansas it began with state, rural-oriented leaders and never penetrated far into the cities. He is cautious in his evaluation of contributors to the movement. While referring to editor William Allen White as "one of the finest specimens" of the Progressive Republicans in the state, La Forte adds that "whether White and his associates were responsible for improved conditions in Kansas is, of course, a moot question." There were other groups, he reminds us, such as Democrats, regular Republicans, and a few Socialists, who also contributed to the enactment of reform measures.

Although La Forte's description of Kansas Progressives on the national scene and his observations on national leaders are brief, they enrich our understanding of the politics of the era. He remarks, for example, that Congressman Victor Murdock's intense effort to help unseat Speaker Joe Cannon sprang in part from that Kansan's having "felt uncomfortable as an unnoticed member of the House." The material on William Howard Taft helps sustain his reputation as a truly bumbling politician. Theodore Roosevelt emerges

as a politician who could perform persuasively but not always with wisdom. La Forte says Roosevelt's speech at Osawatimie, Kansas, in 1910 was the "most momentous delivered by a Republican during the Progressive Era," but of the Roosevelt-led party split in 1912, he states that it is fair to say that the "majority of progressive Republicans in Kansas were sore at heart about what had happened."

The only conspicuous flaw in this admirable study is its inadequate index.

HORACE SAMUEL MERRILL
*University of Maryland,
College Park*

JOHN EVANGELIST WALSH. *One Day at Kitty Hawk: The Untold Story of the Wright Brothers and the Airplane*. New York: Thomas Y. Crowell Company. 1975. Pp. x, 305. \$10.00.

This would be a much better book if the author was not so insistent on grinding an axe. He is convinced that history has been unfair to Wilbur Wright, largely because Wilbur died early (1912) and never told his own story. The thesis is that Wilbur was the genius behind the achievement of powered flight, with Orville in a minor role, but that Orville, who outlived his brother by thirty-five years, allowed a version of the story to circulate that exaggerated his role and suppressed the evidence that would have shown the truth.

Although John Evangelist Walsh has put together a great deal of information, none of it original or unknown, he does not really know how to use it. It does not help that there are no footnotes. There is a section of notes at the end of the text, divided by chapters, but it is not related to the text in any way. Thus it is impossible to relate the data and claims in the text to the sources of evidence listed.

The best example of the author's fuzziness is in the elaborate argument in chapter 10, "High Noon at Kitty Hawk." Walsh contends that the fourth and longest flight on December 17, 1903, Wilbur's fifty-nine second effort, was actually man's first powered flight, thereby eliminating Orville, who had made the first and third flights. Yet in the very next chapter Walsh quotes the telegram sent to Katharine Wright: "Success four flights Thursday morning—." Evidently both the brothers believed they had made four successful flights.

The Wrights and their achievement are a fascinating story, which can well stand retelling. But Wilbur Wright does not need this kind of vindication. His place in history is too secure to require denigration of Orville, as well as of Chanute, Langley, Farman, Blériot, and other great pioneers of flight.

JOHN B. RAE
Harvey Mudd College

ROBERT D. SCHULZINGER. *The Making of the Diplomatic Mind: The Training, Outlook, and Style of United States Foreign Service Officers, 1908-1931*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. 237. \$15.00.

In recent years historians have become more aware of the organizational dimensions of twentieth-century American history and have begun to study the great private and public bureaucracies that dominate our lives. The results of this scholarship have already profoundly affected our view of the American past, shifting attention away from the preoccupations of liberal history toward an examination of the underlying structure of American society. This new organizational approach has become a powerful force in the writing of American history.

Historians of American foreign policy have become conscious of the need to understand the role of bureaucratization and of professionalization in the shaping of American diplomacy. This study is an ambitious attempt to analyze the "collective outlook" of the first generation of professional diplomats, those who entered the foreign service in the early years of the century and rose to influential positions in the 1920s. Drawing on a wide range of manuscript collections and contemporary literature, Schulzinger discusses the emergence of a professional mentality, the campaign for foreign service reform, the training of diplomats, and the sources of their views on world politics. He offers some brief biographical sketches, analyzes the ideas of prominent scholars such as Archibald Carey Coolidge and Paul S. Reinsch, describes the infighting within the State Department in the 1920s over foreign service reform, and explores the tensions that professionalism brought. He argues that career diplomats in the 1920s were realistic in their assessment of international affairs and convinced of the importance of their role in the maintenance of international stability. Though these diplomats constantly sought to explain their unique expertise, they could not define the essence of this "professional mystique." They also failed to convince politicians that diplomacy was the exclusive preserve of diplomats, nor could they strike a satisfying balance between the broad perspective they valued so highly and the growing need for specialization and technical skills.

Despite broad research and frequent insights, this is not a successful book. In seeking to define the "diplomatic mind" for a whole generation of diplomats, Schulzinger reaches too far and produces an inconclusive and fragmented study, not a convincing collective portrait. Lacking the methodology to cope with such a large and difficult question, he often strays from his central purpose. This book is premature, for much of the monographic literature and most of the biographies on

which a synthesis of this scope must rest have not been written. Of all the prominent diplomats mentioned, only a few have been the subject of full scholarly biographies; for the rest we must rely on memoirs, dissertations, or personal papers. The task of exploring such scattered and voluminous sources is huge, and Schulzinger misses some dissertations and ignores rich manuscript collections, such as those of William R. Castle and John V. A. MacMurray. Moreover, the dearth of specialized studies causes him to blur important distinctions and to ignore, for example, the formation of groups of regional experts. In the end, he offers a portrait that is not entirely satisfying either in explaining the beliefs and attitudes that united these diplomats or the differences in background and experience that separated them. The task of comprehending the whole process of professionalization and bureaucratization that diplomats experienced is monumental.

CHARLES E. NEU
Brown University

DICKRAN TASHJIAN. *Skyscraper Primitives: Dada and the American Avant-Garde, 1910-1925*. Middletown, Conn.: Wesleyan University Press. 1975. Pp. xiii, 283. \$20.00.

"Anarchic, protean, subversive, international—however accurately characterized—Dada remains illusive," says Dickran Tashjian of this avant-garde rebellion against art and culture. Beginning during World War I in Zurich, Dada flourished in its nihilistic aftermath as such extravagant individualists as Marcel Duchamp, Tristan Tzara, Louis Aragon, and André Breton assaulted European sensibilities with their fervent manifestoes, outrageous antics, consistent controversy, and calculated attacks on what Dadaists regarded as the fundamental bankruptcy of Western civilization. Deliberately irrational, frenetically effervescent, the whole phenomenon was shortlived and, according to conventional wisdom, left the United States virtually untouched.

Challenging this last assumption in a comprehensive study of the impact of Dada on a segment of the New York avant-garde, Tashjian explores in the process the works of visual artists such as Joseph Stella, Charles Sheeler, Man Ray, Stuart Davis, and Charles Demuth; the poetry of E. E. Cummings, Hart Crane, and William Carlos Williams; and the little magazines of the period—*Camera Work*, *Broom*, *The Soil*, *Contact*, and *Session*. Fed up with traditional values of the academy and impressed with the merits of "straight" photography as advocated in Stieglitz's *Camera Work*, some young American modernists had already begun attacking prevailing cultural definitions of art before the arrival of Duchamp and Picabia about the

time of the celebrated Armory Show. What the Europeans accomplished was the further de-emphasis of physical technique and traditional, even meaningful, content. Thus by enhancing American receptivity to the Dadaist antiart stance, they helped to liberate the New York avant-garde.

Catalytic as well as liberating, Dada—more precisely Picabia and Duchamp—also stimulated interest in American technology and popular culture. To be sure, Crane, Sheeler, Demuth, and others would undoubtedly have discovered this new urbanized, industrialized environment without the help of Europeans. Tashjian is too careful a scholar to make exaggerated claims of Dadaist influence. Of the artists, he admits that only Man Ray became a full-fledged Dadaist; by the same token, he acknowledges that Williams was indeed a vocal opponent of Dada, but he also insists that Williams' preoccupation with creating an American art that was new came out of his "abrasive immersion in the contradictions of Dada." Perhaps so, but there were for Williams, as for most other artists and intellectuals at the time, many other forces at work. One wishes the author had dealt with them, thereby placing Dadaist influence in a richer historical context that, in turn, would have illuminated more fully the problems and preoccupations of the American avant-garde.

Nevertheless, within the limitations he has chosen, Tashjian has made an important contribution to our understanding of this particular facet of American cultural history; in the process, he has demonstrated impressive mastery of visual and literary materials too often ignored by the less-versatile historian.

JANE DE HART MATHEWS
University of North Carolina,
Greensboro

W. ELLIOT BROWNLEE, JR. *Progressivism and Economic Growth: The Wisconsin Income Tax, 1911-1929*. (National University Publications.) Port Washington, N.Y.: Kennikat Press. 1974. Pp. 154. \$10.00.

Tax enforcement in America has led to the Boston Tea Party, the Whisky Rebellion, and the conviction of Al Capone. In *Progressivism and Economic Growth*, W. Elliot Brownlee, Jr. statistically analyzes the Progressive-inspired 1911 Wisconsin corporate income tax and its two-decade impact. He states that Wisconsin "contributed the first comprehensive and effectively administered income tax, served as a model for other states and for the federal government in the search for tax systems more appropriate to a maturing industrial order, and, until the Great Depression, provided the only example of a system of state income taxation that relied heavily on the taxation of corporate profits." The author based his exhaustive research primar-

ily upon Wisconsin corporate income tax returns, the Edwin Witte Papers, and the Thomas S. Adams Papers.

Brownlee shows excellent command of Wisconsin's 1900–30 political intricacies. Robert La Follette and the Progressives attempted to create what the author terms an "agricultural service-state" in Wisconsin. Rural Progressive legislators passed the corporate income tax. The Wisconsin Manufacturers Association and the Milwaukee-based Socialist legislators, who sided with their constituent corporations, strenuously objected to the tax. The Progressive Republican governor, Francis McGovern, won re-election in 1912 by charging that corporate resistance proved the validity of the old Populist shibboleth that industry did not want to pay its fair share of taxes. Even the stalwart Republican governor, Emanuel Philipp, a Milwaukee industrialist, doubled the surtax on corporate incomes in 1919. The Progressive Republican governor, John Blaine (ironically a future RFC official), raised the corporate income tax in 1925 to finance increased state aid to economically depressed rural areas. Brownlee concludes that the Progressives and manufacturers must share responsibility for retarding "the course of industrialization and, in turn, economic growth" in Wisconsin. Progressives instituted the high corporate income tax that impeded economic development. Manufacturers resisted the tax so vigorously that it remained a major Wisconsin political issue for two decades and led to strict enforcement by the Wisconsin Tax Commission.

RICHARD C. HANEY
University of Wisconsin,
Whitewater

ALAN REITMAN, editor. *The Pulse of Freedom: American Liberties, 1920–1970s*. Foreword by RAMSEY CLARK. New York: W. W. Norton and Company. 1975. Pp. 352. \$12.50.

Alan Reitman is associate executive director of the American Civil Liberties Union (ACLU), and this book contains essays tracing the history of civil liberties and civil rights in this country since the ACLU's inception in 1920. It is not a substitute for the scholarly history of the ACLU that has long been needed, but it is a fine introduction to the Union's major achievements and a good summary of the often dramatic developments witnessed in the field of human rights over the past half century.

Excluding a silly exercise in platitudes by Ramsey Clark, the essays are of generally high quality. Paul L. Murphy's examination of the 1920s is solid and reliable although a bit uninspired. He is especially effective in depicting conditions leading to the creation of the ACLU.

Jerold S. Auerbach devotes much of his exhilarating essay on the 1930s to a discussion of an internal dispute over the exclusion of Communists from ACLU leadership, a dispute that led to the expulsion of Elizabeth Gurley Flynn from the board of directors in 1940. Auerbach is sharply critical of the anticommunist policy that emerged. This is the only negative commentary on the ACLU in the book, and Reitman felt obliged to include not overly convincing rebuttals by Roger Baldwin and Osmond K. Fraenkel.

The 1940s are discussed by William Preston, Jr. His analysis is heavily polemical and at times emotional, but he covers the ground well and depicts the emerging red scare late in the decade with eloquence and understanding.

John W. Caughey's well-written essay on the 1950s is entitled "McCarthyism Rampant." Although it avoids most of the deeper questions about the meaning of McCarthyism, it depicts the full range of the second Red Scare and provocatively assesses its damage. Liberals would do well to have this piece in mind the next time they are challenged to discuss exactly what harm McCarthy and his associates caused. Curiously, the author's bibliography is uncritical and out of date.

Perhaps the best article in this volume is by Milton R. Konvitz. His lengthy essay on the 1960s vividly recounts the startling changes in American life produced within this decade of turmoil and tragedy. The author may be criticized for his unswerving, almost naive liberalism. Still, this is a superior example of how to write very recent history, and it deserves wide reading.

Alan Reitman's long and at times tedious conclusion reminds us that this book, for all its scholarly contributions, was designed as a polemic. Reitman equates the ACLU's work with eternal truth. The bad guys—and they often include almost all of the American people—are those who oppose this truth. He pleads for "a truly free and open society." One wishes that he would be more specific about the contents of that society, and one wonders about the role of the much-maligned majority within it.

THOMAS C. REEVES
University of Wisconsin,
Parkside

KEITH L. NELSON. *Victors Divided: America and the Allies in Germany, 1918–1923*. Berkeley and Los Angeles: University of California Press. 1975. Pp. xiii, 441. \$20.00.

America's persistent efforts to moderate Allied enforcement of the Versailles peace agreements following the Senate rejection is one of the anomalies in recent American diplomacy and has received

only limited study. This book concerns the continuation of the American Army of Occupation along the Rhine, and it fully documents the conflicting views of House, Allen, Poincaré, Erzberger, and others during the Wilson period. Harding's policies are less thoroughly treated, and some flaws appear. Secretary Hughes, not Army Secretary Weeks, had become the decisive voice. Hughes was most interested in the growing crisis, but having no official representation on the Reparation Commission, and Rhineland High Commission, and other League commissions, his means of action were seriously limited. He therefore allowed French extremism to run its course to prove its futility, which would then permit the wisdom of moderation.

Senators Lodge and New were also not reliable spokesmen for the administration. Too little attention is given the efforts of Harding and Hughes to overcome Senate obstruction to their moderate course—the use of executive agreements rather than Senate-approved treaties and dilution of the Knox peace resolution. The question of the legality of the Army of Occupation under the Berlin Treaty is also avoided as well as the supplementary agreements used to temporize Allied excesses. The author should also have explained that the withdrawal of the Army of Occupation did not mark the end or even the decline of the American effort to temporize Allied excesses, including the French-Belgium occupation of the Ruhr. Withdrawal only marked a shift in policies that finally restrained Allied excesses, as reflected in the Dawes Plan for payment of American reparation claims and the costs of the Army of Occupation. The restraining influence of the United States was also evident in the Wadsworth, Dawes Plan and other agreements, which called for the restoration of German viability and reintegration and for relief from further Allied sanctions (occupation and so forth) that had finally come to be what the Army of Occupation was all about.

MITCHELL W. KERR
Towson State University

MARGARET SZASZ. *Education and the American Indian: The Road to Self-Determination, 1928–1973*. Albuquerque: University of New Mexico Press. 1974. Pp. xviii, 251. \$10.00.

The influential Meriam Report (1928) redefined the function of the Indian Bureau as primarily educational, and it advocated basic changes in Indian schooling. Its recommendations won the support of all the major reform forces, new and old, in Indian affairs. The progressive educational measures proposed included community day schools, active Indian participation, the elimination or reorganization of boarding schools, and a

flexible curriculum suited to varying tribal needs. Under Commissioners Rhoads and Collier and their directors of education, the eminent progressive educators W. Carson Ryan, Jr. and Willard W. Beatty, the Bureau attempted to implement these policies. A study of this significant experiment has long been needed.

Education and the American Indian makes a good, if limited, start. The best sections cover the period up to 1945. Setting her account within the larger context of progressive education and the rise and fall of the Indian New Deal and focusing on the hitherto neglected work of Ryan and Beatty, Margaret Szasz explores sympathetically both the reforms instituted and the reasons for their eventual “failure.” Included is a critical evaluation of contracting for Indian children in public schools under the Johnson-O'Malley Act.

No attempt is made to assess the impact of Indian progressive education on the new Indian forces that arose in the '40s and '50s with the emergence of the National Congress of American Indians. Similarly, the influence on Bureau education of the broad array of reform movements, Indian and non-Indian, gets insufficient attention. The changing conceptions of such basic ideas as “self-determination” and “cross-cultural education” are also not examined. Fuller and more integrated treatment is needed of the postwar topics discussed in the book. These include the relationship of the termination policy to education, tribally controlled schools and community colleges of recent date, and Indian organizations specifically concerned with education in the late '60s and early '70s. Szasz's pioneering study will no doubt encourage these further lines of inquiry as well as stimulate other interpretations of the educational consequences of the Meriam Report.

HAZEL WHITMAN HERTZBERG
Columbia University

PETER TEMIN. *Did Monetary Forces Cause the Great Depression?* New York: W. W. Norton and Company. 1976. Pp. xiii, 201. Cloth \$8.95, paper \$3.95.

Readers of Milton Friedman and Anna Jacobson Schwartz's monumental *Monetary History of the United States* cannot fail to have been impressed by the compelling narrative and the resourceful display of evidence to support their monetary interpretation of cyclical disturbances in the U.S. economy since 1867. Nevertheless, two questions continue to haunt the reader: Is the Friedman and Schwartz interpretation historically valid? And are their equally persuasive nonmonetary explanations of inflations and depressions? Regrettably, economists and economic historians inspired by Keynesian and post-Keynesian economic theory

have failed to do their historical homework. There is no comparable study by the Keynesians that matches, either in scope or analytical achievement, the study by Friedman and Schwartz.

Peter Temin's new book attempts to fill this lacuna. He restricts himself, however, to a single episode: America's Great Depression from 1929 to 1933. He conveniently simplifies his task by sharply contrasting two rival hypotheses about the causes of the Great Depression—"the money hypothesis" and "the spending hypothesis." The "money hypothesis" associated with Friedman and Schwartz attributes the depression to a sharp reduction in the stock of money resulting from the collapse of the banking system. The "spending hypothesis" has its roots in Keynesian and post-Keynesian income and expenditure theory (of textbook fame) and assigns causal significance to changes in autonomous spending (investment, consumption, or government). In brief, Temin proposes to discriminate between the two rival hypotheses by deducing relevant theoretical implications and designing appropriate statistical tests to measure the presence or absence of these anticipated effects.

He concludes that the "money hypothesis" must be rejected for lack of evidence. Instead, he suggests that the data are consistent with the hypothesis that the demand for money was falling more rapidly than the supply of money during 1930 and the first three-quarters of 1931. He finds strong support for the view that there was a substantial and unexplained decline in real consumption expenditures of approximately three billion dollars in 1930, more than enough in his judgment to explain the subsequent severe contraction of income and employment. He is unsuccessful, however, in his efforts to explain the sharp fall in consumption in terms either of the stock market collapse or the poor harvest of 1929. Temin admits that it is "somewhat unsatisfactory to say that the Depression was started by an unexplained event, but this alternative is preferable to statements that are inconsistent with the data" (p. 83).

This book is an important contribution that imaginatively and skillfully employs the craft of the economist to illuminate an event which continues to baffle both economists and historians. Incidentally, it is a fine example of how the New Economic History can be exploited to great advantage when the problem and the approach are mutually compatible.

EIMUS WICKER
*Indiana University,
Bloomington*

SIDNEY FINE. *Frank Murphy: The Detroit Years*. Ann Arbor: University of Michigan Press. 1975. Pp. viii, 608. \$20.00.

The first of a projected three-volume life and times of Frank Murphy, this book is concerned with Murphy's youth and early career as a practicing lawyer, his judgeship on the Detroit Recorder's Court from 1924 to 1930, and his many problems as mayor of Detroit from 1930 to 1933. The present volume is painstakingly researched and remarkably thorough, chock full of detailed information that should interest historians of politics, law, and urban affairs.

Murphy, born in 1890 to Irish Catholic second-generation immigrants, grew up in a small town in Michigan's thumb into Lake Huron. His father was a self-made man, a lawyer and small-time Democratic politician who drank excessively. As a student at the University of Michigan (LL.B., 1914), Murphy clearly developed political ambitions and was active in student affairs, but he remained a mediocre scholar. As a young man he seems to have been highly principled, vastly enthusiastic, and vigorously athletic, and he emerged as a budding young politician imbued with reformist and progressive ideals of the day. He became a successful lawyer quickly. When first running for office at age thirty-three, he had a lucrative practice and had established himself as one of the city's ablest young attorneys despite more than two years' service as a junior army officer.

On the court, Murphy demonstrated eminent fairness as a trial judge, compassion in setting sentences, enlightenment about bail and probation, and efficiency as an administrator. While on the Detroit court he also built his political base. Blacks, the foreign-born, and unionists honored him for his lack of bias, and middle-class reformers were favorably impressed with his one-man grand jury investigation into municipal corruption. A natural candidate for the special election in 1930 required by a recall of the mayor, Murphy won the nonpartisan election handily, largely because of the good majorities he polled in black and ethnic neighborhoods.

The depression hit the Detroit area fast and hard, with factory employment dropping over fifty percent from August 1929 to August 1930; in November 1932 more than half the labor force was unemployed, and many were working only part-time. No large city was stricken worse during the Hoover years. Mayor Murphy recognized early the magnitude of the depression and the need for public relief of the unemployed, and his was one of the first public voices calling for federal assistance for welfare. The mayor's task was impossible, but his biographer is surely right in concluding that Detroit's suffering would have been much greater if Murphy had not been mayor.

Sidney Fine devotes the overwhelming proportion of his book to Murphy's public career, but he presents some fascinating and puzzling material

about the politician's personality and private life. Fine quite properly describes Murphy's correspondence to his mother while he was in the army as "love letters." Very attractive to women, he had dozens of wealthy girl friends in Detroit, as well as the actress Ann Harding; yet he never married. Murphy was a complex person. He appears to have been unusually ambivalent and contradictory. He had sympathy and easy relations with the poor, but he enjoyed his friendships with the rich; as a young man he was simultaneously an Irish nationalist and enthusiastic about "Americanism" and A. Mitchell Palmer; filled with progressive ideals, he frequently disagreed with good government people.

This is a good start on a major biography. Fine admires Murphy, but recognizes his defects. This book is a model of balance and judiciousness.

DAVID A. SHANNON
University of Virginia

WILLIAM ANDERSON. *The Wild Man from Sugar Creek: The Political Career of Eugene Talmadge*. Baton Rouge: Louisiana State University Press. 1975. Pp. xviii, 268. \$11.95.

EDWARD F. HAAS. *DeLesseps S. Morrison and the Image of Reform: New Orleans Politics, 1946-1961*. Baton Rouge: Louisiana State University Press. 1975. Pp. xii, 368. \$12.95.

These books represent the initial efforts of two youthful biographers whose subjects have not previously been treated in complete biographies. Since Eugene Talmadge and DeLesseps Morrison were products of the New South, these studies will be of special interest to students of Southern history. The authors apparently did adequate research, although their sources were markedly different. Edward F. Haas found much written material and made meager use of oral history, while William Anderson was compelled to depend heavily upon interviews with acquaintances of "Ole Gene" Talmadge. The spirited style of both authors holds the reader's interest.

In many ways the subjects of these books present a study in contrasts. Although Morrison was born in a rural community of southern Louisiana, he soon migrated to New Orleans and lived the rest of his life in that rapidly expanding urban center. Talmadge was born in a rural Georgia environment and spent all of his life in small towns or on a farm. Morrison was repeatedly elected as mayor of New Orleans and served in that position longer than any other person in modern times; Talmadge was elected and re-elected governor of Georgia. Morrison's political life was devoted to the solution of problems confronting his urban constituency, whereas Talmadge served the interests of the farming classes.

When Morrison twice attempted political advancement by running for governor, he was defeated. Talmadge was twice unsuccessful in his efforts to represent his state in the United States Senate. Both of these Southern politicians were racists, but only Talmadge was rabidly so. Both of them strongly opposed integration. Although Talmadge opposed the New Deal and fought the increasing power of the national government, Morrison was more amenable to Washington's expanding regulation of the economy and its enlarging bureaucracy.

Both Talmadge and Morrison had personality conflicts with the boards of educational institutions. As governor, Talmadge fired several educational officials and caused the loss of accreditation for Georgia's colleges and the university. Morrison had differences at times with the city educators, but never took steps as drastic as those of Talmadge. The Georgia governor was always a staunch conservative, while the New Orleans mayor sought to create for himself the image of a reformer. He may have had the image, but frequently the reality of reform was lacking.

The contrast between the two men also extends to their religious convictions. Talmadge was a Protestant in a state that was predominantly Protestant. Morrison was a devout Catholic in a city that was largely Catholic. Both men were devoted to their families.

In appearances they were particularly dissimilar. Although Talmadge was a university graduate with a Phi Beta Kappa key, he was sometimes uncouth in manner, careless in dress, and vulgar in language. The immaculately dressed Morrison was an accomplished conversationalist, suave and debonair.

Both authors were successful in setting their subjects against the issues with which they were confronted during their political careers in the New South. As usual, the Louisiana State University Press did a splendid job in the publishing of these books.

GEORGE OSBORN
Gainesville, Florida

BERNARD BELLUSH. *The Failure of the NRA*. New York: W. W. Norton and Company. 1975. Pp. xiv, 197. Cloth \$8.95, paper \$2.95.

At the hands of historians the National Recovery Administration of 1933-35 has fared badly. Cursed at the time, it has remained the epitome of political aberration, illustrative of the pitfalls of "planning" and deplored both for hampering recovery and delaying genuine reform. Only a few planning enthusiasts have viewed it as the bungling of a good idea, and of these only a few admirers of French and Japanese institutions have looked

upon its corporatist aspects as an unfortunately suppressed thrust toward modern state-building.

In the work under review, Bernard Bellush offers a short, well-written, and well-informed summation of the conventional wisdom. In eight chapters he examines the background and passage of the legislation, the negative impact of the codes, the emasculation of the labor sections, and the decline and demise of the program. His story differs little from that in standard accounts, and the conclusions are essentially those long ago reached by liberal historians. The great mistake, he believes, was in turning political power over to "highly organized, well-financed trade associations and industrial combines."

As a convenient account usable in college courses, Bellush's work has value. It reads well, has a good bibliographical essay, and succeeds in pulling together the stories separately developed by business and labor historians. But for those who expect the Norton Essays to be vehicles for new interpretations or for clarifying interpretive issues, the work will be disappointing. For the most part, it ignores recent challenges to liberal premises and recent "defenses of monopoly" that make NRA economics more understandable. And more surprising, given current interests, it makes no real effort to fit the NRA experience into the larger stories of modern American planning, corporatism, and administrative development.

ELLIS W. HAWLEY
University of Iowa

JOHN BRAEMAN *et al.*, editors. *The New Deal*. Volume 1, *The National Level*; volume 2, *The State and Local Levels*. (Modern America, number 4.) Columbus: Ohio State University Press. 1975. Pp. xv, 341; xiv, 434. \$30.00 the set.

For the third time John Braeman and his coeditors have combined with the Ohio State University Press to give us a useful collection of original essays on twentieth-century America—this time on the New Deal. Volume 1 contains essays by Albert Romasco on the contrasts between Hoover and Roosevelt, James Holt on the New Deal's antistatist critics, Ellis Hawley on the New Deal's relation to American business, Richard Kirkendall on agriculture, Milton Derber on labor, Jerold Auerbach on the legal profession, Raymond Wolters on blacks, John Salmond on Aubrey Williams, Richard Polenber on the years 1937–40, David Brody on liberal reform during World War II, and Eric Solomon on fiction and the New Deal. One can see from this list that the editors opted for established scholars of demonstrated ability and for syntheses on familiar yet still important problems. Little entirely new ground is broken, but almost without

exception the essays constitute comprehensive, sound, and insightful surveys of their subject. Especially useful are Hawley's piece on the conflict between contending strategies for achieving a workable political economy; Kirkendall's review of agricultural policy, which deftly distinguishes the radical from the conservative thrusts of a multifaceted set of federal activities affecting the farm; Wolters' balanced review of how blacks fared under the New Deal; and Auerbach's illuminating study of the role of lawyers, certainly the most interesting and original essay in the book.

The second volume was designed to present some of the new work on state and local politics and policy in the 1930s that James Patterson's *The New Deal and the States* helped to stimulate when it was published a decade ago. Here we have explorations into new terrain, at least geographically. Methodologically, these essays are rigorous, traditional political history. The authors in this volume are younger: Harold Gorvine on Massachusetts, Richard Keller on Pennsylvania, David Maurer on Ohio, Robert Hunter on Virginia, John Moore on Louisiana, Keith Bryant on Oklahoma, F. Alan Coombs on Wyoming, Michael Malone on Montana, James Wickens on Colorado, William Pickens on New Mexico, Robert Burton on Oregon, Bruce Stave on Pittsburgh, and Lyle Dorsett on Kansas City.

Allowing for the complexity of the story, the editors are right to conclude that "the result of this new interest [in state and local history] has been further to downgrade the significance of the New Deal as an instrument of fundamental changes in American life and society" (vol. 2, p. ix). The New Deal's local impact was of course substantial in certain respects, bringing some political realignment, broadening the policy agendas of lower-tier governments, and integrating their welfare and economic policies somewhat into a national design. As a whole, however, these essays indicate that change did not run deep or carry very far. Democratic strength ebbed more quickly at local levels than it did in Washington, and the season of reform was shorter. Local Democratic factions and leaders—one hesitates to call them parties—usually did not offer anything like the programmatic coherence or idealism of the Democrats in Washington, and in any event their resources were even thinner.

Since historians do not often ask exactly the same questions, it is no surprise that these volumes do not produce any decisive realignment of accepted scholarly opinion. Most of the authors tried to indicate the impact of their own research upon the running argument between those who think that the New Deal should have made more changes in American life and those who are im-

pressed by the barriers it faced. No scholar in these volumes places himself unflinchingly at either extreme, but on the whole they tend to underscore the deep conservatism of American social structure and values and thereby reinforce what might be called the Leuchtenburg, as opposed to the Bernstein-Zinn, view of matters. Roosevelt appears as a restraining influence in a relatively open situation in the essay by Brody, and to some extent that of Wolters. If one wants to ponder other and more deeply rooted sources of resistance to social reform in the 1930s, there is imposing evidence in the essays—for example, on Massachusetts, Virginia, Oklahoma, and Colorado, or in Polenberg's review of the surge of conservatism and nativism that ended the New Deal in the late 1930s—and at many other places in this two-volume work. If these essays were to be dedicated to one of Roosevelt's most influential contemporaries, it should not be Aubrey Williams but Martin Dies.

OTIS L. GRAHAM, JR.
University of California,
Santa Barbara

DONALD HOLLEY. *Uncle Sam's Farmers: The New Deal Communities in the Lower Mississippi Valley*. Urbana: University of Illinois Press. 1975. Pp. xv, 312. \$14.50.

Contemporary critics considered the New Deal's Resettlement Administration (RA) and its successor, the Farm Security Administration (FSA), as radical. Others dismissed them as Communist-inspired, collectivistic communes. Conversely, some latter-day historians have contended that the New Deal served the interests of the middle class, was too conservative, and ignored lower-strata Americans. In his admirable study of New Deal farm communities in the Lower Mississippi Valley (Region 6, comprising Arkansas, Louisiana, and Mississippi), Donald Holley presents a convincing thesis. Neither the RA nor the FSA, he maintains, was particularly radical, but their efforts to cope with agrarian distress marked a new departure. His final estimate is that the program was rather successful, given its inherent drawbacks. This moderate evaluation is not a mark of vacillation, but rather a careful judgment based on thorough research in primary materials.

National figures, such as Henry A. Wallace and Rexford G. Tugwell, are not ignored, but primary attention is properly directed to such regional administrators as William Reynolds Dye, Marvin T. Aldrich, and E. B. Whitaker. Most of them were products of Southern land-grant colleges, especially Mississippi State.

The bitter struggles of the Southern Tenant Farmers' Union, led by H. L. Mitchell and Henry

Clay East, are skillfully related. Such conflicts helped secure passage of the Bankhead-Jones Tenant Farmers Act of 1937. The RA was later abolished and replaced by the FSA under Will Alexander.

The programs were intended to deal with rural poverty and land reform. The federal government bought submarginal land, converted it to other uses, and resettled the families on better land. To achieve the goal of owner-operated, family-sized farms, low interest loans were made available on a long repayment schedule.

Holley points out that the program discriminated against blacks and that too few black administrators were hired. He concedes that only a beginning was made in dealing with the great mass of poverty-stricken farmers, black and white. Yet he recognizes the importance of the beginning and demonstrates its benefits.

Holley's style is clear and unadorned. He sifts through a mass of documentary evidence and gives it meaning. What he has to say is illuminating and important.

WILLIAM W. ROGERS
Florida State University

MICHAEL S. HOLMES. *The New Deal in Georgia: An Administrative History*. (Contributions in American History, number 36.) Westport, Conn.: Greenwood Press. 1975. Pp. xi, 364. \$14.95.

In this study Michael S. Holmes assesses the impact of conditions in Georgia on the recovery efforts of eight New Deal agencies: the Federal Emergency Relief Administration, the Civil Works Administration, the Works Progress Administration, the Public Works Administration, the National Recovery Administration, the Agricultural Adjustment Administration, the Resettlement Administration, and the Farm Security Administration. Relying principally on project files in the National Archives, Holmes methodically describes the organizational structures and goals of these agencies and analyzes the manner in which they spent their funds. He concludes that New Deal programs were most effective when their state administrators received sufficient latitude to deal with local problems and conditions.

The New Deal in Georgia provides some interesting anecdotes but little else that is not available in James T. Patterson's *The New Deal and the States*. As a whole, the narrative is excessively detailed, but at certain crucial points it fails to supply enough information. For example, although Holmes feels that Georgia Governor Eugene Talmadge's opposition hindered the recovery programs, he provides no evidence that conditions improved when Eurith D. Rivers, a vocal New Deal supporter, succeeded

Talmadge. Because this is a narrowly focused administrative history, the broad conclusions drawn in the final chapter are both incongruous and unwarranted. Holmes credits the New Deal with expanding the horizons of Georgia's "common men" and making them realize that as voters they could "demand that their leaders build schools, roads, or anything else they wanted" (p. 320). There is little evidence, either in this book or in the political behavior of Georgians in the post-New Deal era, to support such a judgment.

JAMES C. COBB
University of Maryland,
College Park

EVERETT CARLL LADD, JR., with CHARLES D. HADLEY.
Transformations of the American Party System: Political Coalitions from the New Deal to the 1970s. New York: W. W. Norton. 1975. Pp. xxv, 371. Cloth \$12.50, paper \$4.95.

This is probably the most important book available for understanding the American party system, at least as that system exists in the electorate and in the grand coalitions that constitute each major political party. The principal author is a political scientist at the University of Connecticut who has written extensively on American political history. The associate author, a Connecticut Ph.D., was in charge of data analysis; the book is solidly based on all of the major sources of survey data since the beginnings of the Gallup Poll in 1936.

The book begins with a concise review of five recent alternative theories on where the party system is going: a new Republican majority, a new Democratic majority, a search for the center, an era of partisan decomposition, and a renewed New Deal. The authors find some merit in the arguments underlying most of these alternatives and see their own object as a single integrative statement. They have not found a party realignment and do not think that one is occurring. But they are confident that we have had a transformation of the party system in the years since 1960.

Their summary statements produce the following picture: "There is no majority party in the presidential arena." "There has been an inversion of the old New Deal relationship of social class to the vote. . . . This inversion follows from very basic changes in the structure of conflict in American society, and is likely to be long-term." "The Republican coalition . . . is weaker now than at any time since . . . the Great Depression." "A 'two-tier' party system has emerged, with one set of electoral dynamics operating at the presidential level, and yet another in sub-presidential contests." "A major new cohort of activists has assumed vastly in-

creased importance in the electoral arena" (pp. 26-27).

Part 1 of the book deals with the formation of the New Deal party system and its major elements, what happened to it up to 1960, and the disintegration of the New Deal system in the South beginning in 1948. Part 2 is on the party system since 1960, and it begins with an exceptionally well-done chapter on the nature of postindustrial society. The shifting party coalitions are then traced in detail, followed by thorough analysis of how the opposite and equally "unnatural" land-slides of 1964 and 1972 could have happened. The book concludes with an anticipation that we can expect more of the same.

PAUL T. DAVID
University of Virginia

ROBERT SOBEL. *N.Y.S.E.: A History of the New York Stock Exchange, 1935-1975*. New York: Weybright and Talley. 1975. Pp. xi, 398. \$15.00.

The author notes the relative lack of competent critics for a work of this sort; insiders are not given to meaningful comment, and outsiders, who are accustomed to sensing faults in historical treatments, are not sufficiently familiar with the subject matter. I fall into the second group, but immediately protest that Robert Sobel teaches me much about the operations of the Exchange. The book was written "with the lay reader in mind." This includes more than refraining from technical jargon, for the author does not spare space in spelling out functions, practices, and situations otherwise puzzling. The language of the text is lively throughout, and the tables are invitingly clear. This is not the first, though it is the most sustained, of Sobel's explorations in this field of business history.

The New York Stock Exchange imperfectly registers the country's metabolism. Much of this volume is of necessity given to a running account of Big Board day-to-day and month-to-month performance. But many pages are devoted to economic, political, military, technological, and other developments in America and the world that conditioned actions on the Exchange. Sometimes the author looks at events through the eyes of buyers and sellers of securities, but just as often he describes the passing scene as an objective historian. An attractive example of the latter is his characterization of the Eisenhower presidency, with its combination of confidence and conservatism that was wise enough to continue certain proven New Deal policies. Even here the end was not as happy as the middle period, reminding us that we are always to expect incessant fluctuation.

The behavior of the Exchange was frequently

due more to its own internal contentions or to the operating style of its chief executive, than to governmental investigation or policing. The Securities and Exchange Commission loomed larger in the New Deal organization chart than in the discipline of "the street," even when reputed dangerous activists were appointed to its membership.

The history of the Exchange spans generations. Succeeding ones have short memories. Newer participants, Sobel says, regarded the crash of 1929 as a remote calamity like the Civil War, and in one way or another they compelled experienced seniors to cater to their venturesome demands. This prevented decay, although at a cost.

It is evident throughout these pages that the author did prolonged digging in the archives of the Exchange, at Broad and Wall streets and especially at the warehouse on Hudson Street. The mass of the materials at the latter would repel a less diligent investigator. He has not stopped with written and printed records of many kinds but has sought the opinions and recollections of knowledgeable persons in the financial district. It will be a while before another scholar attempts such an inquiry.

BROADUS MITCHELL
Rutgers University

CABELL PHILLIPS. *The 1940s: Decade of Triumph and Trouble*. (The New York Times Chronicle of American Life.) New York: Macmillan Publishing Company. 1975. Pp. xi, 414. \$12.95.

Although written by a former New York Times Washington correspondent and a reporter knowledgeable about the national political scene, this is nonetheless a disappointing book. It is not surprising that Phillips has not researched primary sources; a more crucial deficiency, however, is his failure to have done extensive reading in the recently published secondary literature. His research consists principally of New York Times files and news stories, selected interviews, and a smattering of books (principally memoirs) published during the 1950s and early 1960s. The result is a superficial and impressionistic analysis of American society and politics during the 1940s, which contains numerous minor errors of fact. His limited research further contributes to the author's often simplistic rendering of complex issues. Moreover, developments that were not recognized in the contemporary press accounts as notable but which proved to be important legacies—for instance, the institutionalization and expansion of the presidency, the creation of the CIA, and the expansion of FBI surveillance authority—are barely discussed. These omissions detract from Phillips' book, for he contends that the 1940s constituted a

transition decade, one marking a division between an old and a new order, particularly as this relates to the nation's role in international affairs, changes in values and priorities, and the emerging conception of the role and responsibility of the federal government. Lastly, the author's frequent resort to sweeping, unsupported generalizations about dominant values and public opinion reduce the value of this book for the serious scholar. This is the kind of book that will send the "new social" historians into apoplexy.

Where this book can be commended is in Phillips' masterful account of the important trends and developments of the decade. In one sense, this is a readable and condensed New York Times almanac, reporting statistics in a readable and insightful form; detailing changes in productivity, consumption, residential patterns, and life styles; and discussing the important international developments, political campaigns, legislative debates, and congressional hearings. The book is extremely well written and easy to read, and Phillips' recounting of interesting anecdotes provides insights into the mood of the period.

ATHAN THEOHARIS
Marquette University

W. AVERELL HARRIMAN and ELIE ABEL. *Special Envoy to Churchill and Stalin, 1941-1946*. New York: Random House. 1975. Pp. xii, 595. \$15.00.

As Roosevelt's lend-lease representative in London from 1941 to 1943 and as ambassador to the Soviet Union from 1943 to 1946, W. Averell Harriman wielded extraordinary influence. A confidant of Roosevelt and Churchill, with unusual access to Stalin, he became an intermediary among the Big Three, a man entrusted to represent the views of one to the other. In the early days of the Truman administration he played a vital role in the formulation of policy toward the Soviet Union.

Now in his mid-eighties, Harriman has at last offered a full account of his service in World War II. The fruit of close collaboration with the journalist Elie Abel, *Special Envoy* is neither memoir nor biography. Rather, drawing heavily on Harriman's personal papers and recollections, Abel has written a history of Big Three diplomacy from the ambassador's perspective and offered his judgments on men and events.

The book makes interesting reading, but it will disappoint those who have kept up with the recent literature on the period. There are some fascinating glimpses at major personalities, and there is information about day-to-day contacts with the British and Russians that does not appear in the documents. But there are no "bombshells" here: much of the ground covered is familiar; many of

the documents included are well known. Moreover, Harriman's comments on his contemporaries are remarkably discreet, even bland. He comes down hard only on Eisenhower, whose naiveté he found "astounding," and on James Byrnes, whose secretiveness he deeply resented. He was very close to Roosevelt and Churchill, but he offers little fresh insight into their personalities and attitudes. He comments at length only on Stalin. The Grotian moralist in Harriman was obviously appalled by Stalin the "murderous tyrant," but the man of affairs was deeply impressed by the Soviet leader's "high intelligence" and "fantastic" grasp of detail. "I found him better informed than Roosevelt, more realistic than Churchill, in some ways the most effective of the war leaders."

Harriman's observations on the origins of the cold war raise some serious problems. He vigorously defends Roosevelt's and Churchill's attempts to promote Eastern European governments that would be "friendly to, but not the creatures of, Moscow." He concludes that the "fact that we tried and failed left the main responsibility for the Cold War with Stalin, where it belongs." It may be questioned, however, whether such governments were within the realm of possibility and how Harriman's "firm but friendly quid pro quo approach" could have produced them. The ambassador chides Truman for his tongue-lashing of Molotov and for the abrupt cessation of lend-lease in May 1945. Yet it was Harriman who stirred Truman's wrath with talk of a "barbarian invasion of Europe" and who masterminded the change in lend-lease policy. *Special Envoy* obscures Harriman's role in Truman's adoption of a "tough" policy, and the Harriman of this account appears more moderate than the Harriman of 1945, at least as he is revealed in the documents.

In short, the book is nicely crafted, but because it comes so late it contains few surprises. One hopes that Harriman and Abel will not delay in carrying the account through the Truman, Kennedy, and Johnson years.

GEORGE C. HERRING
University of Kentucky

CHARLES P. ROLAND. *The Improbable Era: The South since World War II*. Lexington: University Press of Kentucky. 1975. Pp. 228. \$11.95.

The issue of Southern exceptionalism, like that of American exceptionalism, goes back to the seventeenth century, and *The Improbable Era* is yet another contribution to the literature dealing with that old Puritan theme. This study is a macroappraisal of changes in the South since World War II. It is comprehensive, extending from economics and politics through education and religion to

music and the visual arts, and it is both extensively researched and written in a straightforward style.

If the early chapters of the book are weak, the last two are gems of scholarly precision. In chapters 10 and 11 Roland concentrates on the theme of change amid persisting tradition, a theme long associated with C. Vann Woodward, whom the author frequently cites. He is also concerned, inferentially, with the Veblen-Ogburn "cultural lag" phenomenon. Among other things, Roland suggests that Southern history is forgetful but ultimately unforgiving, for, as he amply demonstrates, the mythic and defensive South is still very much alive.

The strength and variety of the author's arguments make impossible a capsule summary of the book; the very clarity and ease of style help conceal the density of thought and concentrated labor that went into it. As for the shape of the book, that was apparently dictated by the easy availability of censuses and other related data.

Social scientists are likely to feel that the book contains too much narrative description and too little interpretive analysis. The bibliographical note is excellent, but historians will find the documentation for the various chapters woefully inadequate. Still, if it is true that Roland's reach exceeds his grasp, one cannot fault him on his acute sensitivity to historical nuance and complexity or on his keen sense of historical irony.

Marx once wrote that "the tradition of all past generations weighs like a nightmare upon the brain of the living." In this work Roland has reconfirmed that proposition as it relates to the contemporary South.

FRANCIS M. WILHOIT
Drake University

THOMAS M. CAMPBELL and GEORGE C. HERRING, editors. *The Diaries of Edward R. Stettinius, Jr., 1943-1946*. New York: New Viewpoints. 1975. Pp. xxviii, 544. \$12.50.

Like many officials departing from government service, Edward Stettinius, former undersecretary and secretary of state, as well as ambassador to the United Nations, carted off his papers—daily summaries of events, transcripts of phone conversations, memoranda of discussions, correspondence with ambassadors and Washington officials, and reports to the president. Drawing upon this substantial collection at the University of Virginia, Thomas M. Campbell and George C. Herring, who as graduate students organized the materials and used them in dissertations, have edited this useful volume. At many points it supplements the *Foreign Relations* series for 1943-46, and sometimes

even the State Department records at the National Archives.

The Diaries, like the larger collection, reveals a cautious, insecure man, reluctant to offer judgments and unable to push for policies beyond his beloved United Nations. Despite the editors' hopes of raising scholars' estimates of Stettinius, this volume confirms the consensus: he was a man of limited ability who operated far from the center of power on most important issues; policy swirled past him, and he watched it develop and change.

Analysts of the cold war will find considerable detailed information on the creation and operation of the United Nations, especially on the San Francisco Conference in 1945 and the Iranian dispute in 1946. But they will be disappointed that *The Diaries*, like the papers themselves, contains little rich information about the formulation of American policy toward the Soviet Union, especially on the key problems of Eastern Europe, Germany, and the Far East. Among the more interesting, previously unpublished documents are those that reveal that on the eve of Yalta Roosevelt told senators that the Russians "had the power" in Eastern Europe, that a break in relations was undesirable, and that America's economic position (primarily lend-lease) was not "a bargaining weapon of any strength"; he informed Stettinius a week later that he preferred dealing in person with Stalin on the Russian loan; W. Averell Harriman, ambassador to the Soviet Union, reported shortly after Yalta that the conference proved that Soviet policy was directed at maintaining "intimacy between the three Great Powers"; Secretary of State James Byrnes admitted in September 1945 that he had thought of telling Molotov at the London Conference that the United States would use the atomic bomb only to halt aggression and maintain peace; and Truman concluded by October that the United States "had perhaps four to ten years" to work out international control of atomic energy.

BARTON J. BERNSTEIN
Stanford University

GEIR LUNDESTAD. *The American Non-Policy towards Eastern Europe, 1943-1947: Universalism in an Area Not of Essential Interest to the United States*. Tromsø: Universitetsforlaget; distrib. by Humanities Press, New York. 1975. Pp. 653. \$22.50.

"The main point of this study," concludes Geir Lundestad, a young Norwegian historian, "is that Washington was never able to develop any consistent policy towards Eastern Europe" (p. 429). In demonstrating that proposition, the author explores the basic elements and motivating factors of the United States' wartime policy; gives detailed accounts of American interests and actions regard-

ing Hungary, Czechoslovakia, Poland, Romania, and Bulgaria, as well as brief notes for comparative purposes on policy toward Finland and the Baltic states; considers the eventual peace treaties with the Axis satellites; and assesses the military and economic leverage available to the United States in support of its policies. A notable inclusion is the useful survey of American attitudes regarding the various schemes for Eastern European federation.

The picture that emerges is clear if unsurprising. Washington's approach was dominated by "universalism," which subsumed the pursuit of a world organization, democratic governments, and economic multilateralism. These principles, publicly enunciated in the Atlantic Charter and in the Yalta Declaration on Liberated Europe, were eventually nullified by the immutable determination of Stalin to secure a subservient sphere of influence on the Soviet Union's western approaches. The marginal strategic and economic importance of Eastern Europe for the United States, the irreconcilability of the twin Rooseveltian goals of an Eastern Europe democratically governed as well as genuinely friendly to the Soviet Union, the American inclination to neatly distinguish between military strategy and political objectives, and Anglo-American and United States unilateralism in dealing with defeated Italy and Japan, all conspired against application of the universalist model to Eastern Europe. What was left was a declaratory policy whose diplomatic pursuit in 1945-46 proved a bitter exercise in futility and marked the irrevocable disintegration of the Grand Alliance.

The author claims that in stressing the "constant contest between universalism and its modifiers" he surpasses existing "traditionalist" and "revisionist" accounts that allegedly fail to discern the complexity of United States policies toward Eastern Europe. He finds little evidence to support the revisionists' economic determinism, notes that Washington was generally most sympathetic to the political center in Eastern Europe (where the indigenous communists were in a distinct minority), and concludes that "even if universalism with its democratic-multilateralist elements may be seen as an expansionist ideology, this did not necessarily mean that it conflicted with the wishes of the Eastern European peoples. Usually it did not" (p. 420). Washington's policy of "idealistic self-interest" proved impotent in the face of the "one crucial factor"—the presence of the Red Army in a region that in the American world view was of secondary priority.

Lundestad's style is clear and his research impressive. He has explored all the relevant American documentary collections. The unnecessarily

fragmented structure of the work suggests a dissertation in need of streamlining, but the study provides a thorough account of a critical and often contentious sphere and phase of United States foreign policy. The promise of novelty through a synthesis of traditionalist and revisionist approaches is belied by the author's conclusions. As with John Lewis Gaddis' outstanding analysis of the origins of the cold war, the outcome of dispassionate scholarship is only a marginally amended elaboration of orthodox historiography.

BENNETT KOVRIG
University of Toronto

JOSEPH P. LASH, biographical essay and notes by. *From the Diaries of Felix Frankfurter*. Assisted by JONATHAN LASH. New York: W. W. Norton and Company. 1975. Pp. xiii, 366. \$12.00.

Joseph Lash's fine introductory biographical sketch of Felix Frankfurter (the "Brahmin of the Law"), which is ninety pages long and well annotated, endows this book with lasting value. It is written with the meticulous care of Frankfurter's own work and is a thought-provoking estimate of the judge's character and contributions. There is a curious dualism between Frankfurter's two prime roles as the behind-the-scenes political adviser and as the Supreme Court justice holding forth for judicial self-restraint in the realm of personal liberties as well as economic matters. Lash sees Frankfurter as the brilliant immigrant youth who became a profound American patriot, worshipping a panoply of heroes: Holmes foremost, but also Brandeis and Stimson, and beginning in the 1930s, Franklin D. Roosevelt. Frankfurter gained a reputation for radicalism through his early defense of the liberties of such social rebels as Sacco and Vanzetti, and he reluctantly abetted Roosevelt in his struggle against the Supreme Court. On the Court, Frankfurter was being consistent in insisting that the judiciary should not interfere in economic matters. He proved himself more a wartime patriot than a radical in matters of civil liberties. Lash speculates fruitfully on the reasons for these attitudes; the diaries illustrate some of his points.

Beginning with his arrival in Washington in 1911 as a young assistant to Stimson through his years on the Supreme Court, Frankfurter kept, at least intermittently, a diary full of detail and feelings. Parts he withheld, others he destroyed, and some pages were stolen from the Library of Congress. The sections Lash has located and reproduced are an interesting sampler, valuable for their glimpses of Frankfurter and his renowned contemporaries. There are only a score of pages on the Taft administration, and even fewer on two squabbles with President Lowell of Harvard in 1929 and 1933. At

the heart of the book are well over a hundred pages on 1943, the year in which Frankfurter records his emphatic differences with Justices Douglas and Black and his considerable involvement in the issues and intrigues of wartime Washington. A span of less than a hundred pages covers the period from the autumn of 1946 to the spring of 1948, with emphasis more on the Court than the Truman administration. These sections on the 1940s are primarily of value to the legal historian interested in tracing the discussions leading to several significant decisions and in the interplay among the justices. The vehemence of some of Frankfurter's comments may well indicate, as Lash suggests, that he used the diary as a safety valve.

As a mirror of Washington, the diary is useful but by no means extraordinary. In 1943 Frankfurter was seldom involved directly with President Roosevelt, and like Justice Brandeis earlier, he was most significant as a sage removed from the center of action, feeding suggestions to his devoted and influential coterie. In the Truman years his chief access to inside knowledge was Dean Acheson, with whom he regularly walked to work. The diary is primarily useful, therefore, as a supplement to Frankfurter's legal writings, his reminiscences, and Max Freedman's edition of his enormous, adulatory correspondence with Roosevelt.

FRANK FREIDEL
Harvard University

ALEXANDER L. GEORGE and RICHARD SMOKE. *Deterrence in American Foreign Policy: Theory and Practice*. New York: Columbia University Press. 1974. Pp. xii, 666. Cloth \$17.50, paper \$6.95.

This book provides a comprehensive assessment of both the theory and practice of deterrence in American foreign policy from 1948 to 1963 through the perspective of eleven limited-war case studies. Some of the more important cases examined include the Berlin Blockade, the outbreak of the Korean War, the intervention of the Chinese in that war, the Hungarian revolution, and the Cuban missile crisis. The authors conclude that deterrence theory has been seriously deficient as a guide to effective foreign policy in that period because it was insufficiently rooted in real historical data, oversimplified, deductive rather than inductive in method, and more prescriptive than analytical. Arguing that deterrence theory is not a self-contained strategy, the authors call for greater reliance on historical studies by decision makers, more willingness to use a multifaceted process of give and take, and especially the application of pressure for a more positive and long-term world view of where America should be going.

In the first part of the book the authors discuss the nature of contemporary deterrence theory,

suggesting that it is well developed at the strategic level of all-out nuclear war but comparatively underdeveloped at lower levels of policy. At no level have the advocates of this theory given much attention to historical cases of deterrence. Methodologically, deterrence theory remains untested and therefore ought to be used sparingly, if at all, and decreasingly as multipolarism increases in the world.

The second part of the book applies deterrence analysis together with historical explanations to evaluate the deterrence process and explain what happened in the eleven instances. The authors conclude that the Berlin Blockade is not a good example of successful deterrence but a failure from the standpoint of the Western powers because the Soviets achieved policy modifications in Western Europe and West Germany they would not have achieved otherwise. On the Chinese intervention in the Korean War, the writers conclude that neither side wanted an escalation and that both the Chinese and the Americans miscalculated the motives and the intensity of the other power's commitments because of wishful thinking, shifting goals, and Washington's desire to eliminate the North Korean regime. The Cuban missile crisis was caused by the Soviets' misperception of John Kennedy's character, their tendency to discount serious warnings as politically motivated, and American lack of receptivity to early clues of a missile deployment owing to a high priority the White House gave to relaxations of tensions with the Soviet Union. Kennedy did correctly perceive Khrushchev's willingness to act rationally when under extreme pressure, however, and the result of the 1962 missile crisis was a major turning point in the cold war toward greater détente. The other cases are examined in much the same fashion.

The final part of the book attempts a reformulation of deterrence theory toward a broader, multifaceted process that would include greater emphasis on classical diplomacy, more discriminating use of a variety of other means of influencing adversary behavior, and the acceptance of a policy based less on threats and more on the recognition of a limited role for appeasement and negotiations.

George and Smoke have written a thoughtful, instructive, and interdisciplinary account that should be useful to policy makers for some years to come. Their call for greater use of history in foreign policy making should be widely applauded by historians generally, but their conclusions will probably not fully satisfy revisionists. There are few new data here, but the analysis is exceptionally well done and should be useful to anyone working on matters where deterrence theory is of some importance.

JAMES L. CLAYTON
University of Utah

LAWRENCE S. WITTNER. *Cold War America: From Hiroshima to Watergate*. New York: Praeger Publishers. 1974. Pp. x, 403. Cloth \$12.50, paper \$6.95.

In *Cold War America* Lawrence Wittner writes an appealing, but often aggravating, omnibus of America's political history from the end of the Second World War to the fall of Nixon. Aiming at the nonspecialist and being self-consciously critical of "consensus" history, Wittner shows the strengths and weaknesses of the survey approach. To his credit, the author succeeds in discussing a great number of complex issues in an entertaining manner. This stylistic asset becomes a liability, however, where Wittner avoids or oversimplifies a complex historical problem for the apparent sake of the narrative. Unfortunately, this tendency occurs at numerous times in the text. Wittner distorts the "non-consensus" view of American foreign policy by accepting it so uncritically. Few serious revisionist historians would agree with Wittner's portrayal of cold-war origins; in particular, few would accept his premise that the accession of Truman in 1945 marked an abrupt shift from the policies of the Roosevelt administration.

The section on the beginning of the cold war is especially disappointing for its disregard of the many new sources on that subject. This neglect leads the author to rely heavily upon Truman's own notoriously self-serving memoirs—ironically so since the former president is himself clearly one of the villains of Wittner's book. There are other significant omissions or distortions: American policy toward the Middle East receives surprisingly little attention, whereas America's interest in Vietnam is dubiously portrayed as being based upon economic motives. The condition of blacks in America gets superficial treatment, and feminism receives barely a paragraph. In his defense, however, Wittner's predominant concern is with foreign and domestic policies, and his forays into social and cultural history (at one point he suggests a connection between "McHale's Navy" and escalation in the Vietnam War) appear more in the way of interesting sidelights.

It is perhaps because of Wittner's skill as a reductionist that the best parts of his book deal with the recent domestic events. The domestic consequences of foreign policy are too often ignored in survey histories, though not here. Wittner's brief section on McCarthyism and the McCarran Act is a useful corrective to the myths that continue concerning the anticommunist hysteria of the 1950s. His treatment of the 1968 and 1972 elections, based largely upon journalistic accounts, is also reflective and convincing. Finally, the author's bibliographic essay is a compendium of major secondary literature.

Wittner's book hardly breaks new ground with

regard to either sources or interpretations of the cold war; nor, in fairness, was that its intent. It is a worthwhile synthesis of the modern period with a new and welcome emphasis upon the domestic scene.

GREGG F. HERKEN
California Polytechnic State University

DAVID EDWIN HARRELL, JR. *All Things Are Possible: The Healing & Charismatic Revivals in Modern America*. Bloomington: Indiana University Press. 1976. Pp. xi, 304. \$10.95.

In the decade following World War II all kinds of religion received unusually warm and often unexpected acceptance. One group of revivalists who benefited from this outpouring of enthusiasm consisted of more than twenty men and women from pentecostal backgrounds who preached a gospel stressing bodily healing. In the tradition of revivalism they developed idiosyncratic styles, often controlled their finances badly, were charged with self-serving and corruption, but retorted with fierce individualism and competitiveness. They broke free from their denominations, founded magazines, and attempted to create radio and television networks. In the style of the 'fifties, they often traveled abroad, bringing healing to Argentina or Java. People who attended their campaigns threw away their crutches, saw for the first time, or felt cured of cancer. But, when physicians examined these people, the doctors failed to support the revivalists' fantastic claims. After a period of decline in the 1960s, these charismatic revivalists entered another era of prosperity and success in which their message was not so much healing but the gift of tongues and an emphasis on prosperity as a gift of God. These revivalists are virtually unknown outside the circle of their admirers, but as usually has happened in revivals of this sort, one managed to stamp his name and personality on the movement as a whole and to exemplify what the movement stood for. In this case the man was Oral Roberts.

DAVID EDWIN HARRELL, JR. *All Things Are Possible*: collection of fugitive sources to bring together this account of a movement that has not previously received scholarly treatment. As a biographical dictionary or a reference work, it will serve historians well, but Harrell has failed to set the movement into a context in which these revivalists can be understood by the student of either social or religious history. Furthermore, he has been so timid in judging the careers of the revivalists that the reader cannot ascertain what the author means to say. He writes, for instance, that "few popular writers understood the vast differences between the ministries of Jimmy Swaggart and David Ter-

rell"; but I felt no more enlightened about that difference than the maligned "popular writers" after having read Harrell's descriptions. One certainly does not want a simplistic score card of the good and bad guys, but if a historian fails to establish a point of view, he produces, as I fear Harrell has, only a catalog.

ERNEST R. SANDEEN
Macalester College

HAROLD JOYCE NOBLE. *Embassy at War*. Edited with an introduction by FRANK BALDWIN. (The Studies of the East Asian Institute.) Seattle: University of Washington Press. 1975. Pp. xxiv, 328. \$12.50.

Harold Joyce Noble was first secretary of the United States embassy in South Korea from August 1949 to January 1951. *Embassy at War* is his personal account of the first three difficult months of the Korean War. Noble vividly portrays the frustrations of an embassy on the move and its relations with the Korean government and army, the United States military, and, to a much lesser extent, the State Department. There are few startling revelations, but by providing for the first time a perspective from the embassy, Noble makes several interesting observations. Contrary to the impression given in Douglas MacArthur's *Reminiscences*, the South Korean army was not a disorganized rabble following the North Korean capture of Seoul. For the most part, the ROK army fought bravely and had a number of able generals. With that sense of omniscience that has characterized American military leaders since the Second World War, the American generals thought they knew exactly how to fight a war in Korea and adopted an arrogant attitude toward their Korean counterparts. It was the embassy staff, especially Ambassador John Muccio and Noble himself, who convinced the American command to adopt the successful program of integrating Korean soldiers into the army's undermanned units and using Korean police battalions to detect and fight infiltrators. Besides serving as liaison with the ROK government and army, the embassy profoundly affected the course of the war by maintaining the morale of Koreans and Americans shocked by the invasion and initial defeats—its chief role in these crucial summer months—and by preventing President Rhee and the Korean politicians from interfering with the prosecution of the war. Noble, who knew Rhee since 1938, was assigned the delicate task of persuading the president to leave the front. Noble stresses that South Korea was unprepared for war and did not provoke it; Rhee always remained dubious of American support.

Noble was raised in Korea by his Presbyterian

missionary parents. He taught Far Eastern history at the University of Oregon and at colleges in Korea and Japan before turning to journalism and government service. Throughout these years, Noble was devoted to the cause of Korean independence, unity, and freedom, to MacArthur's occupation policies in Japan, and to hard-line anti-communism and containment of Soviet Russia. *Embassy at War*, however, is generally a well-balanced and lively account based on Noble's notes and correspondence. Noble blasts the State Department for even thinking of dissolving the South Korean government following elections in liberated North Korea, but he fails to appreciate the international complications that confronted America's Korean policies. The chief weakness of this account is Noble's plastic image of Rhee, who is presented as the patriotic leader throwing temper tantrums whenever he was overruled by Washington and his own cabinet.

Frank Baldwin has provided an excellent sketch of Noble and has admirably edited the long manuscript that the diplomat left at the time of his death in 1953. Baldwin's explanatory notes, which contain correspondence from members of the embassy staff, are particularly valuable.

Embassy at War is a partial but suggestive account of the Korean War. It serves as a reminder that a comprehensive diplomatic history of the conflict has yet to be written.

NOEL PUGACH
University of New Mexico

GARY W. REICHARD. *The Reaffirmation of Republicanism: Eisenhower and the Eighty-Third Congress*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1975. Pp. xv, 303. \$14.95.

In this well-organized and clearly written study Gary W. Reichard attempts "to define . . . Eisenhower's political views, expertise, and overall impact" by analyzing his relationship with the Eighty-third Congress. The choice of the Eighty-third Congress for a case study is logical because it was Eisenhower's initial, and only Republican, Congress. Reichard limits his investigation to four major areas: "foreign policy, fiscal and economic policy, welfare policy, and policy related to power and resource development," which, he correctly believes, reveal the range of Republican ideology. He devotes the first fifth of the book to a political profile of the Republican bloc in Congress, including both incumbents and members of the class of 1952. To determine the degree of support these Republicans gave to Eisenhower's programs, Reichard applies quantitative techniques to 135 roll call votes on "Eisenhower issues." He also relies

heavily upon manuscript collections, especially those at the Eisenhower Library and the Dulles Oral History Project at Princeton.

The book's conclusions offer no major surprises. Eisenhower and the Eighty-third Congress did not usher in a new Republicanism; they returned to and reaffirmed "traditional Republicanism." Reichard demonstrates that the Eisenhower administration proved "unerringly conservative" regarding public power, resource development, and fiscal policies. Its objectives included a balanced budget, lower taxes, diminished responsibilities for the federal government, and encouragement of private activity. Eisenhower's programs toward social security, labor and farm legislation, health, and housing likewise "adhered closely to orthodox Republicanism."

Foreign policy, Reichard admits, is the exception to the pattern of traditional Republicanism that Eisenhower championed. Focusing on the Yalta resolution, the confirmation of Charles E. Bohlen as ambassador to the Soviet Union, and the proposed Bricker amendment, the author concludes that Eisenhower "internationalized" his party. But, he argues, "aside from foreign policy, there was no 'Eisenhower revolution'"; he could have insisted just as easily that aside from domestic policies, Eisenhower reshaped his party's ideology.

Reichard's assessment of Eisenhower as a party leader is solid, although the author admits that "incontrovertible evidence . . . frequently is missing." The president desired to lead his party and did so successfully, using "a combination of conciliation, persuasion, and political maneuver," while keeping in mind he needed Democratic votes in Congress.

Within the guidelines he set, Reichard has produced a careful study that deserves a respected place on the short shelf of Eisenhower historiography.

KEITH W. OLSON
*University of Maryland,
College Park*

LEWIS J. PAPER. *The Promise and the Performance: The Leadership of John F. Kennedy*. Foreword by JAMES MACGREGOR BURNS. New York: Crown Publishers. 1975. Pp. xi, 408. \$9.95.

This is a curious study, one that is useful and better balanced than some of the products of passionate journalistic partisanship that have already appeared, but one that is nevertheless deeply flawed. Some will see it as a more judicious appraisal than Henry Fairlie's analysis. Like that book, it also concludes that Kennedy's performance did not fulfill expectations. The most impor-

tant difference is that Paper stresses Kennedy's confrontation with the realities of the presidency rather than the cynical manipulation of expectations.

Paper's Kennedy comes remarkably close to the earlier view presented by James MacGregor Burns, at least to the more critical parts of that earlier work. Here, too, Kennedy emerges as a capable, well-intentioned politician, one with a taste for power and with confidence in his own talents. Unlike Burns, however, Paper concentrates on the presidency and his use of power. In the process he confirms many of the criticisms made in the past. More than any president before Nixon, he was concerned with personal image. His obsession with the art of the possible precluded more effective leadership, especially during the civil rights struggles of 1962 and early 1963. Paper comes to the reasonable conclusion that too often "his ideals were compromised by his understanding of the American and international political systems" (p. 346).

For all its value, the book is disturbing. Composed of five massive chapters, it largely ignores essential chronological relationships and concentrates on problems as though they occurred in a vacuum. So Kennedy's support of the communications space satellite program, acknowledged as an attempt to ingratiate himself with business, is viewed apart from the showdown over steel prices, a confrontation that occurred just a few months earlier.

But most jarring are the qualities that will tempt the specialist to question Paper's mastery over his subject. The entire work is laced with naïveté, including the statement that Kennedy was concerned with his television appearances *even after* entering the White House and the observation that "the president should always want to do the right thing" (p. 7). His discussion of Kennedy's 1946 victory ignores the importance of the family background in the Boston area and his appeal to war veterans, let alone the recent publicity over PT-109. It will be unfortunate if such weaknesses obscure the contributions that Paper makes to an analysis of Kennedy's conduct of the presidency.

HERBERT S. PARMET

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RICHARD MAXWELL BROWN. *Strain of Violence: Historical Studies of American Violence and Vigilantism*. New York: Oxford University Press. 1975. Pp. xiii, 397. \$13.95.

LEE KENNETT and JAMES LAVERNE ANDERSON. *The Gun in America: The Origins of a National Dilemma*. (Contributions in American History, number 37.) Westport, Conn.: Greenwood Press. 1975. Pp. x, 339. \$12.95.

Violence in American life is an ordeal, a dilemma, a curse, and most of all a historic fact. Until President Johnson set up the National Commission on the Causes and Prevention of Violence (1968), however, it had been little studied. The Commission's report helped put violence into some historical perspective, and now Richard M. Brown, a member of the Commission who is currently teaching at William and Mary, adds to the literature.

"Our history," Brown declares in his introduction, "has produced and reinforced a strain of violence, which has strongly tinted our national experience." Indeed, "repeated episodes of violence, going far back into our colonial past, have imprinted upon our citizenry a propensity to violence" (p. vii). Brown attempts to prove his point with a detailed examination of violence in the American Revolution, putting special emphasis on South Carolina extremism. He devotes the middle third of the book to vigilantism and the last third to black-white violence and "The Violent Region of Central Texas: Land of Lyndon B. Johnson." The appendix contains lists of colonial riots, revolutionary riots, vigilante movements, slave insurgencies, black-white riots, and lynchings.

Altogether, it is a dreary story. Brown does little in the way of analysis; he is more concerned with recounting violent acts than understanding them. There is a great deal of information in this book, which is characterized by diligent scholarship buttressed by extensive notes. But horrifying as each individual event may be, they lose in the text their ability to shock when murder follows murder on page after page.

There is no concluding chapter, but Brown makes some generalizations within the body of his text. First and foremost, we are all guilty. "The patriot, the humanitarian, the nationalist, the pioneer, the landholder, the farmer, and the laborer (and the capitalist) have used violence as the means to a higher end" (p. 36). But some are more guilty than others. Vigilante violence "has been socially conservative. . . . It has been used to support the cohesive, three-tiered structure of the American community with its upper, middle, and lower classes" (p. 4), and "established groups have been quick to resort to violence in defense of the *status quo* they dominate" (p. 5). Brown contends that "outlaws and the hostile lower people . . . constituted a 'contraculture.' They . . . wished to upset the social structure" (p. 105). The conclusion seems too pat, and in any case it would appear that outlaws on the American frontier were hardly a group out to upset capitalism; more than anyone else they wanted the stagecoaches and the trains to run on time, carrying lots of money.

Brown contends that Lyndon Johnson's Viet-

nam policy "was a throwback to the central Texas ethic of violent self-defense" (p. 238), and he devotes nearly seventy pages to an account of violence in the region. To the objection that Walt Rostow, McGeorge Bundy, and others pushed the Vietnam policy and that they did not come from a region with a violent tradition, Brown replies that they were responding to a totally different tradition, "a tradition of paternalistic American intervention in the world that flourished among the Anglophile elite of the eastern U. S." (p. 288). Brown insists that Johnson personalized the war, a result of his upbringing. But what then of Nixon, who also personalized the war and who outbombed Johnson, despite his California Quaker background?

The major difficulty with *Strain of Violence* is the absence of any comparative work. Brown proves the obvious—that there has been a great deal of violence in American life—but he does not explain it, nor does he compare it to other cultures. It has been said that we are no more violent than any other people; what is different about Americans is that we are more likely to have a gun handy when we get angry and thus more likely to commit murderous violence. Certainly this is the conclusion of Lee Kennett and James L. Anderson in their study, *The Gun in America*.

The Kennett and Anderson book is a model historical study, well written, deeply researched, objective. They begin with an extended discussion of the European experience with privately owned guns, and throughout their examination of gun laws in the United States they continue to refer to contemporary European practices. This is also a refreshing study in that the authors are not advocates; they do a first-rate job of presenting the arguments of both the National Rifle Association, which they find less influential than the popular press believes, and the antigun groups.

Are Americans more violent than other people? Kennett and Anderson take pains to point out that sheer numbers of guns at hand does not always lead to lethal violence: "Israel, which has survived by becoming an armed camp, is a case in point. Switzerland, whose military constitution requires that able-bodied men keep weapons in their homes, is another" (p. 249). The entire subject of violence in America, in short, needs more study. These books will help push that study along.

STEPHEN E. AMBROSE
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CANADA

IVAN AVAKUMOVIC. *The Communist Party in Canada: A History*. Toronto: McClelland and Stewart Limited. 1975. Pp. x, 309. \$5.95.

Ivan Avakumovic recounts in detail the history of the Communist party of Canada (CPC) from its origins after World War I up to the early 1970s. He describes the twists and turns of party policy through the Popular Front period of the 1930s, the Second World War and after, the crisis of de-Stalinization in the 1950s, the relationship between the CPC and various ethnic groups, particularly the Finnish, Ukrainian, and Jewish immigrants in the 1920s, and the infiltration of youth groups, tenants' associations, cultural societies, and other front organizations. Special attention is devoted to the attempts of the CPC to gain control of the trade unions, an effort that was temporarily successful.

Throughout most of its history the party was under the leadership of Tim Buck who retired in 1962. Under his guidance the CPC adhered rigidly to the Stalinist line and vigorously opposed Trotskyite and Maoist deviations. In spite of all its efforts, however, the party never won more than one to two percent of the popular vote in national elections. The reasons for the failure of the CPC to make headway in Canada were varied: intermittent harassment by governments; the constant factionalism within the party; the difficulty of implanting Stalinist policies in a society different from that of Eastern Europe; and, above all, the existence of a strongly entrenched democratic party in the CCF-NDP, which attracted most of the left-wing intellectuals and political activists in the trade unions. In Canada, as in other countries, Khrushchev's denunciation of Stalinism in 1956 created a crisis in the party from which it was slow in recovering. The future of the party today is uncertain.

Although this book is a good factual presentation of the party's history, it is repetitious in places. A major criticism is that it is poorly structured and reads more like a chronology of events rather than an analytical study.

HERBERT F. QUINN
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DAVID JAY BERCUSON. *Confrontation at Winnipeg: Labour, Industrial Relations, and the General Strike*. Montreal: McGill-Queen's University Press. 1974. Pp. x, 227. Cloth \$10.00, paper \$5.00.

David Jay Bercuson provides a new perspective on the Winnipeg general strike of 1919 in *Confrontation at Winnipeg*. Earlier scholars, including D. C. Masters in *The Winnipeg General Strike* (Toronto, 1950), viewed the strike's causes as largely political, but Bercuson stresses the importance of the "industrial background" and the "additional strains" placed on labor by World War I.

Bercuson traces the roots of the general strike

back to 1906, when a strike against the contract machine shops failed because of an injunction. Drawing upon a wide variety of published and manuscript sources, he then proceeds skillfully to explain the growing alienation of labor in Winnipeg from government, employers, and even the Trades and Labor Congress of Canada, supposedly controlled by AFL "choreboys" from the east. By 1918 "radicalism was firmly in the saddle." This radicalism contributed to western Canada's support of the One Big Union.

Although interest in the OBU confirmed that many workers viewed society through "polarized glasses," Bercuson convincingly argues that they remained confused both as to their objectives and their resources. Despite their oratory, the strike leaders were neither revolutionary nor foreign directed. Speaking in the "round tones of north England or the thick burr of the Clydeside," they believed in 1919 that a general strike was the best means of achieving a traditional objective, the right to bargain collectively. The lack of revolutionary fervor doomed the general strike, usually seen as a political weapon, to failure. The strikers were unwilling to permit society to collapse and themselves became "the chief strikebreakers."

Since *Confrontation at Winnipeg* is presented as a reinterpretation of the general strike, it is somewhat disappointing that Bercuson did not explicitly discuss the historiography of labor relations in western Canada and the importance or uniqueness of his own approach. Especially surprising is his failure to analyze, or even to recognize, Masters' book on the general strike. The inclusion of this work would have been appropriate if for no other reason than to illustrate the need for another book on the topic.

BRIAN L. BLAKELEY
Texas Tech University

LATIN AMERICA

RICHARD GRAHAM and PETER H. SMITH, editors. *New Approaches to Latin American History*. Austin: University of Texas Press. 1974. Pp. xiv, 275. \$8.75.

This is among the most important and useful books on Latin American history to appear in the past decade. The nine essays are not only good historiographies of a significant range of Latin American topics, but they are also "think pieces" on how historians might proceed in their search to understand these topics. Suggestive approaches to the study of colonial bureaucrats, nation-state formation in Spain and Latin America, nineteenth-century landowners, political party alignments, regionalism, slavery, immigration, psychohistory, and political legitimacy are presented.

The editors write that the book was "designed to reflect and to promote the conceptual and methodological changes that have already started to affect the study of Latin American history." They and their colleagues have admirably fulfilled this purpose. Each one of these essays makes a solid contribution, a rarity in a collective work. All skillfully make use of a specific research interest to focus on broad conceptual and methodological issues, and all utilize to some extent disciplines other than history. Two-thirds of the essays deal in a broad sense with some aspect of politics. The authors are interested in the socioeconomic context of politics, the linkage between socioeconomic groups, and the exercise of political power.

As impressive as this book is in most respects, it nevertheless has several shortcomings. Many of the authors, for example, refer to the central problem of the underdeveloped status of research and writing on Latin America, by which they mean that compared to the United States and Western Europe there are poor data, a lack of basic monographs, and few conceptual works to guide research. The book makes an important contribution to overcoming the conceptual lag, but none of the authors dwell sufficiently on the implications of this underdevelopment and what might be done about it.

A second and related problem is that seven of the nine authors focus their attention on elites. Some of them do so in part because of the underdeveloped state of scholarship in Latin America, but also on philosophical grounds. As Richard Graham sums up, "From the point of view of the historian, then, there is a marked economy of effort in tracing the course of change by studying the behavior of those in the upper levels of the political structure. This is especially so for areas that, like Latin America, have not been thoroughly studied even by old-fashioned political historians." This, it seems to me, is a false economy and a distortion of history. Elites are important, but so are nonelites, and the one group is not intelligible without the other.

One approach that is of critical importance to an understanding of nonelites in Latin America is quantitative history, yet quantitative history receives little attention in this book. Michael Hall laments the lack of manuscript census data, directories, and tax records for São Paulo and suggests that "it seems unlikely that research of the sort associated with Stephan Thernstrom and other 'new urban historians' in the United States can be duplicated for São Paulo." This is only partially true. There is no lack of available aggregate data on Latin America in published censuses, government reports, and records of private groups. And these can be exploited successfully by quantitative methods.

The shortcomings, as well as all of the issues of this fine book, are subjects for debate. It is a must for professors and students of Latin American history.

SAMUEL L. BAILY
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C. R. BOXER. *Women in Iberian Expansion Overseas, 1415-1815: Some Facts, Fancies and Personalities*. New York: Oxford University Press. 1975. Pp. 142. \$12.75.

Four lectures delivered at Bryn Mawr in late 1972 are here published in somewhat expanded form. The work is the first general survey of the history of women in the Spanish and Portuguese empires, and though uneven in its coverage, it provides a large amount of information and documentation. As is well known, an older interpretation made much of the contrast between the predominantly male colonization of Hispanic America and the family life of the English colonies, but the simplistic versions of this contrast have been progressively revised in recent decades. We now recognize that the situation in the Spanish and Portuguese empires was one of far greater complexity and variety than was once supposed. Boxer first offers a series of vignettes from the Portuguese colonies in Africa, including stories on white women, Moorish wives and concubines, mulatto servant girls, captives, and occasionally whole companies of female soldiers. For Spanish America, where recent studies by James Lockhart, David Brading, and others have added so much to our knowledge, we have a great range of situations and types. Boxer emphasizes the strong position of colonial Spanish and Creole ladies in *encomienda* and property inheritance. Seclusion of women occurred in both Brazil and Spanish America (the former more than the latter), but the practice is not to be equated absolutely with subordination, nor did it eliminate women from a functioning social or economic role. More Portuguese women went to Brazil than to either Africa or Goa, and it seems to be in Goa especially that the more celebrated excesses in colonial social relations prevailed. A final chapter, more analytic and less episodic than the others, considers the Iberian version of the idea of inherent female inferiority, particularly in relation to the cult of Mary.

CHARLES GIBSON
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MENDEL PETERSON. *The Funnel of Gold*. Boston: Little, Brown and Company. 1975. Pp. xi, 481. \$15.00.

This is a popular history of Spain in the Caribbean from the time of Columbus to the mid-eighteenth

century, and Mendel Peterson tells the story with imagination and enthusiasm. The book describes the many obstacles—pirates, storms, shipwrecks, and virulent disease—that hampered Spanish monopolization of the New World. It is an adventure story filled with accounts of heroism and villainy played out against the background of the struggle between Spain and its enemies, England, Holland, and France, to control the wealth of the New World. In addition to the heroic, the mundane aspects have not been neglected. There are detailed descriptions of the ships that plied the seas between Spain and its American empire, their routes and their cargoes of treasure. Of particular interest are the chapters dealing with recent underwater exploration of sixteenth-century shipwrecks. Many interesting illustrations, including photographs of objects uncovered by the underwater explorations, complete this volume.

For his sources the author depends almost entirely on printed primary accounts (collections of documents, chronicles, contemporary writings) with only scattered references to archival documents, although he indicates that he undertook investigations in the Spanish and British archives. The quotations from the printed materials are unnecessarily long, since they have been drawn from readily available and known sources (Bernal Díaz del Castillo, Vázquez de Espinosa, Thomas Gage, and others). The bibliography contains references to most of the standard older accounts and chronicles, but it includes few recent monographic studies.

The book is more or less dependable as to facts and events but laden with personal generalizations on the Spanish, the English, and the Dutch. There are opinionated comments on historical personages. The old clichés and stereotypes are present; for example, the Spanish soldier is described as being "convinced of his own skill, haughty and full of self-righteousness, greedy and cruel for the love of it" (p. 9). Is this not the villainous Spaniard of the "Black Legend"? Does the author really expect us to believe that "the incurable dilatoriness of the Spaniard also contributed to confuse the schedules of the American fleets" (p. 69)? Individuals fare no better—the royal judge Francisco de Bobadilla was, according to the author, a "jealous small-minded petty tyrant able to disgrace one of the truly great men of history [Columbus]" (p. 7). Here is a world peopled by good guys and bad guys with the stereotyped Spaniards cast as foils for the English and Dutch interlopers.

Certainly there is more to the history of Spain in the Caribbean than its struggle against natural phenomena and foreign enemies. Viceroy, governors, settlers, pirates, and interlopers move across these pages in a colorful procession, but Peterson has not been concerned with the broad and deep

circumstances—social, political, and economic—of the world in which they lived, or for that matter the institutional structure of the Spanish empire. The book is pure and simple narration.

There is no doubt that "sword and swagger" volumes of this kind will continue to sell books for large commercial publishing companies because they seemingly provide entertainment for the general reading public. Moreover, it is quite clear that the author wrote this volume because of his fascination with the period and region, and that is an admirable reason. But it must be recognized that such books contribute little to the advancement of serious historical research.

RUTH PIKE
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PHILIP WAYNE POWELL. *Soldiers, Indians and Silver: North America's First Frontier War*. Reprint. Tempe, Ariz.: Center for Latin American Studies, Arizona State University. 1975. Pp. xv, 317.

MAX L. MOORHEAD. *The Presidio: Bastion of the Spanish Borderlands*. Norman: University of Oklahoma Press. 1975. Pp. xiii, 288. \$9.95.

Arizona State's paperback edition of Philip Powell's *Soldiers, Indians and Silver* is simply a reissue of the book originally published by the University of California Press in 1952. With the exception of a new four-page foreword, the texts are identical. For a review of this work, the reader may consult C. G. Motten's laudatory evaluation in the *American Historical Review* (58 [Jan. 1953]:404-05). It is one with which I fully concur.

What Max Moorhead has undertaken is to take Powell's work as a starting point—it covers the story of the advance of the Spanish frontier northward in Mexico only in the sixteenth century—and carry the account to the end of the eighteenth century. The first 110 of the 271 pages of his work are devoted to this task. An equal number of pages were then allotted to a more detailed examination of the presidio itself in terms of its physical construction, its personnel and administration, and its relationships with other frontier Spanish settlements, towns, ranchos, and Indian reservations.

The role of the presidio as a military fortress in the advancing Spanish frontier in North America was crucial, and it has long called for a thorough investigation. Moorhead's work goes a long way toward satisfying this need, at least so far as the central *meseta* of Mexico is concerned. His account is particularly full and valuable in its treatment of the extreme northern presidial line, stretching from Sonora to Texas, which formed the culminating development of the system. Moorhead points out that the line of presidios along the Rio Grande

and spaced westward across Chihuahua and Sonora is virtually identical with the present Mexican and United States border. It marked the northern edge of effective Mexican settlement, with detached salients in Texas, New Mexico, and California, and was so recognized in the Treaty of Guadalupe Hidalgo. Moorhead is also to be commended for his description and analysis of the problems of graft and maladministration these distant frontier posts presented to the central authorities of New Spain. The efforts made in the *reglamentos* of 1729 and 1772, and by the establishment of the *Provincias Internas* as an administrative frontier agency to cope with these problems, are also well covered.

Moorhead deliberately excluded the two Californias from his survey because he felt the problems they presented were dissimilar. For the same reason, presumably, he did not attempt to cover the Mexican mainland west of the Sierra Madre Occidental: the present states of Sinaloa and southern Sonora. This is unfortunate because the interesting collaborative function of the presidio and mission as paired frontier agencies is best illustrated in those areas. This aspect of the presidio's contribution to the frontier system is only lightly touched upon in this book.

It is also unfortunate, in view of the general excellence of this work, grounded as it is on primary documents, that the author chose to rely on secondary works entirely for his treatment of the Yuma area. The authors of these secondary works had not gone to the sources either, except in a most perfunctory way. As a result, the treatment of the Yuma story the author presents is not only worthless, it is almost entirely erroneous. Readers who wish to get at the truth should consult *legajo* 517 of the Guadalajara *ramo* in the Archive of the Indies.

For the most part, though, Moorhead's work is thoroughly researched. The footnote citations are copious and rewarding, and the bibliography is a full one, though there are some curious omissions, Ocaranza and Priestley, for example. The book is also beautifully bound and well printed. There are excellent maps and illustrations; the most fascinating of the latter are twenty-one contemporary sketches of frontier presidios that Moorhead found in the British Museum.

EDWIN A. BEILHARZ
University of Santa Clara

WARREN G. KNEER. *Great Britain and the Caribbean, 1901-1913: A Study in Anglo-American Relations*. East Lansing: Michigan State University Press. 1975. Pp. xvii, 242. \$12.50.

This Creighton University professor's work has appeared almost simultaneously with Lester D.

Langley's study of Caribbean diplomacy, which perhaps suggests a renewed interest in this particular area. The two books complement each other, rather than compete. Langley's work, written with an American focus, covers the whole period from 1776 to 1904, whereas Warren Kneer's study is confined to the years 1901-13 and has a British perspective.

Kneer shows that the Anglo-German cooperation in the Venezuelan intervention of 1902-03 was prefaced by the cooperation of five European powers in the Guatemalan dispute of 1901, staged largely at Lord Lansdowne's initiative. He emphasizes that the Admiralty, nervous about the growth of the German navy, became increasingly reluctant to disperse its strength to meet the requirements of gunboat diplomacy and virtually abandoned the West Indian station in 1904 in favor of concentration in home waters. Thus Britain therefore was in no position to contest the Caribbean area with the United States even if it had desired to do so, which it did not. Lansdowne and Grey readily accepted the political thrust of the Monroe Doctrine, but they continued to fear its possible economic consequences.

The author examines in some detail Anglo-American relations vis-à-vis Cuba, Santo Domingo, Panama, and Colombia. He then turns to the problems in Central America created by the Dollar Diplomacy of Taft and Knox. Finally, he traces the decline of Anglo-American cooperation, which resulted in part from the quarrel between the United States and England over the Panama Canal tolls. He concludes that the United States never quite lived up to the implications of the Roosevelt Corollary; it proved to be only a part-time policeman, and an unreliable bill collector of European debts. But both British investments and commerce greatly expanded in the Caribbean area during these years.

Kneer's work, though short, is based on solid sources. Interesting and informative, it illuminates how and why Britain acted as it did in this relatively minor, though complex, corner of the world.

WILBUR DEVEREUX JONES
University of Georgia

EVALDO CABRAL DE MELLO. *Olinda restaurada: Guerra e açúcar no nordeste, 1630-1654*. São Paulo: Editora da Universidade de São Paulo. 1975. Pp. 390.

At the Newberry Conference on Colonial Brazil in 1969, C. R. Boxer called for a moratorium on writing about Dutch Brazil in that so much of value had already been produced by H. J. Wätjen, F. A. Varnhagen, José Honório Rodrigues, Gonçalves de Mello, and Boxer himself. We are in some ways fortunate that the author of this book

did not heed this advice. Evaldo Cabral de Mello, a Pernambucan and an amateur historian only in the sense that he is not an academician, has produced a solid and exciting book that not only presents new materials, but also offers an interesting set of revisionary hypotheses concerning the motives of the Luso-Dutch War in Brazil and the means by which it was carried out. The author's diplomatic career has allowed him to visit the major archives of Spain and Portugal as well as those of Pernambuco and to make use of little-exploited collections such as *Guerra Antiga* at the Arquivo Nacional de Simancas. This new material, coupled with a broad reading of the major social and economic studies of the European and Brazilian seventeenth century, makes this an especially valuable and well-rounded monograph.

The central thesis of the work is that the Luso-Dutch War was a sugar war not only in the commonly held sense that it was a war for sugar, but rather that it was also a war made possible by sugar. The sugar sector had to mobilize its human and economic resources to win the war, but when victory was accomplished in 1654 the means of winning the war—taxes, forced migration, confiscation, commercial disruption—and the subsequent general period of stagnation in the Atlantic economies created a crisis for the northeastern sugar economy from which it did not recover until the nineteenth century. Although others have noted the Brazilian contribution to the war, Cabral de Mello makes it painfully clear that the almost totally Brazilian financing and manning of the War of Restoration (1645-54) was more responsible for the decline of the sugar industry in the northeast than the destruction of the military campaigns themselves. After 1654 poor international prices for sugar and a lack of capital for rebuilding in the northeast created a closed circle in which the crown was forced to continue its heavy taxation because of low revenues, and the fiscal burden prevented capital accumulation and reinvestment in new or expanded mills. Thus, while the book is primarily about the Luso-Dutch War, its implications are much broader, and these are largely but not always recognized by the author.

Forgoing a chronological recounting of the events, the author has organized *Olinda restaurada* into eight topical chapters that deal with aspects of the central theme described above. Most interesting and original are three chapters concerning the financing of the war, the recruitment of troops, and the provisioning of the armies. These chapters use detail, rather than quaint descriptions of minutiae, to support the major theses. Cabral de Mello demonstrates, for example, how the desire to save oxen needed by the sugar mills led to a

slaughter of cows to feed the troops and that this in turn later caused a shortage of oxen. Such details are set in a broad perspective that includes comparisons of the Luso-Brazilian armies to those described by Geoffrey Parker in Spanish Flanders and correlations between the timing of the revolt and the Atlantic crisis as analyzed by Ferdinand Braudel, Pierre Chaunu, and Ruggerio Romano. The falling price of sugar in Amsterdam, more than "nativism" or religious enmity, caused the "War of Divine Liberation."

Olinda restaurada is proof that seemingly well-worn topics can yield important new insights to serious research and historical imagination. Cabral de Mello must now have a place alongside those authors mentioned at the outset of this review.

STUART SCHWARTZ
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Twin Cities*

ABRAHAM F. LOWENTHAL, editor. *The Peruvian Experiment: Continuity and Change under Military Rule*. (Written under the auspices of the Center for Inter-American Relations.) Princeton: Princeton University Press. 1976. Pp. xvi, 479. Cloth \$22.50, paper \$6.95.

The only way to hint at the range of this book on the endeavors of the Peruvian military, beginning in October 1968, to forge a new Peru and a new Peruvian is to indicate some of the conclusions of the contributors. First, though, I shall hazard a general evaluation. The essays with two exceptions sustain an unusually high level of excellence and are based on exhaustive research. Lowenthal's opening chapter disappoints, being far less perceptive and probing than the introductory essay David Chaplin provides to his edited book *Peruvian Nationalism: A Corporatist Revolution* (1976), a more helpful book than the one under review for the general reader interested in an interpretive framework and willing to dispense with massive detail. Notwithstanding its challenging insights, Julio Cotler's analysis of the new mode of political domination is marred by ideological rigidity and repetitious Marxian jargon. This will commend the essay to many readers whose judgment may be sounder than mine.

Richard Webb finds the military government has accomplished a little, but no more than that, toward stepping up progressive income redistribution. David Collier demonstrates that in its policies toward squatter settlements the officers have drawn on policies of previous governments. The difference is that the new regime is moving, with some success, to incorporate the urban poor into corporative control systems aimed at undermining traditional parties.

Dealing with the transformation of the rural sec-

tor, Susan C. Bourque and David Scott Palmer stress the diversity that has impeded accomplishments and resulted in vastly differing levels of social mobilization, and they conclude that by increasing the number and level of demands placed upon it, the regime is jeopardizing its ability to carry out objectives. On a related topic, Colin Harding writes that once the political decision was made to carry out serious agrarian reform, "the conflicting interests and claims upon the land released by the process itself made either increasing radicalization or tougher repression inevitable" (p. 221).

Education, as described by Robert S. Drysdale and Robert G. Myers, has become a tool of cultural pluralism with the objective of maintaining the Quechua language and culture alongside the Spanish. At the university level, government attempts to mobilize the academic community have been a notable failure. Shane Hunt skillfully shows that rather than throwing out foreign investors the government has sought to tame multinational capital and to make it work better for national interests. Foreign investors now view the regime as a sometimes capricious and always formidable adversary at the bargaining table.

Peter T. Knight describes the attempt to introduce worker-managed firms within a pluralistic economy, including important private and state sectors. Noting widespread objections to the experiment, he foresees trouble, as do virtually all the authors. In the concluding essay Jane S. Jaquette finds that the government bureaucracy has not been able to adjust to the role of entrepreneur, that the officers are trying to build a new society on the basis of an unprecedented and perhaps fatally weak political combination (Lowenthal properly drew attention to the lack of widespread and enthusiastic support for the government), and that it is not yet clear whether they "will be able to create a sound institutional base to give substance to the Revolution."

FREDRICK B. PIKE
University of Notre Dame

MARTIN WEINSTEIN. *Uruguay: The Politics of Failure*. Westport, Conn.: Greenwood Press. 1975. Pp. xvii, 190. \$13.50.

Focusing on twentieth-century Uruguay, the "proverbial Switzerland of South America," Martin Weinstein sets out to explain why a nation regarded as vastly different from and superior to its neighbors so quickly fell and came to resemble them. After establishing the theoretical framework, the author describes and analyzes Uruguay's development during the first half of the twentieth century. He discusses the impact of José Batlle y Ordóñez and his egalitarian political phi-

losophy, *Batllismo*, as well as that of his most notable political opponents: the famous *Blanco* leader Luis Alberto Herrera, the *Federación Rural*, and Benito Nardone's *Liga Federal de Acción Ruralista*. The latter group promoted a conservative vision of Uruguayan society based on religion, land, free trade, and idealization of rural life. Weinstein argues that this *ruralista* philosophy ultimately had a greater impact on shaping post-1930 Uruguayan politics than *Batllismo*. The author explains that these conservative groups rose to power, in part, through the clever use of the politics of co-participation and the failure of the *Batllista* state-directed economic programs.

Examining the history of co-participation reveals its ability to contain conflict as well as its tendency to divide the nation into two competing political parties each representing the "patria" for its constituents. The parties garnered support by controlling political patronage in the vast state bureaucracy and government enterprises; Weinstein stresses that this particularistic and clientelistic brand of politics did not offer Uruguay either the leadership or mechanisms necessary to exercise power in an effective national manner. To reinforce his theory of the corporatist and divided nature of Uruguayan society, the author utilizes demographic, economic, and educational data to demonstrate the lack of factors promoting geographic and economic integration and social mobility. In the final chapter, Weinstein argues that the failure of both *Batllismo* and the politics of co-participation opened the way for today's military-dominated authoritarian regime, which emerged in a climate of economic chaos, political stasis among traditional parties, and mounting urban guerrilla warfare. As in so many other nations, Uruguay's leaders chose class and privilege over the development of an egalitarian national community.

This compact and informative book provides an incisive analysis of Uruguay's "politics of failure" and subsequent "fall" in the 1960s and '70s. It is the best book in English on twentieth-century Uruguayan political history. Included is an excellent bibliography of primary and secondary sources in both English and Spanish. Even though *Uruguay: The Politics of Failure* will have most appeal for Latin Americanists, other area specialists will find it useful for comparative purposes.

CYNTHIA LITTLE
Philadelphia, Pennsylvania

MANUEL GARCÍA SORIANO. *El periodismo tucumano (1817-1900): Ensayo de investigación sobre un aspecto de la cultura de Tucumán durante el siglo XIX*. (Cuadernos de Humanitas, number 38.) Tucumán: Universidad Nacional de Tucumán. 1972. Pp. 113.

This work is the product of love and dedication. For over fifty years Manuel García Soriano patiently labored to locate and identify all the newspapers published in the Argentine province of Tucumán between 1817 and 1900. In recent times he also had the help of his students at the Universidad Nacional de Tucumán. His undertaking was an important one, not only for the history of journalism but also for the political, cultural, and economic history of the province. He was able to identify 148 newspapers, and he has classified them as follows: political, literary, educational, scientific and technical, official, student (elementary and secondary), Catholic, Masonic, humorous, satiric, occasional, union, and immigrant. He also identifies women's publications and publications appearing outside the provincial capital. Over half of the political newspapers cataloged appeared after 1885, and over half of the others after 1890. Between 1888 and 1892 political fervor led to the founding of nineteen newspapers, the largest number for any four-year period. These had an ephemeral existence, for all but two of them are known only because they are cited in other newspapers. Newspaper references rather than extant copies actually account for slightly over half of the political newspapers listed. The description of the others is based on isolated numbers, facsimiles, or incomplete sets at the Biblioteca Alberdi, Sociedad Sarmiento, Instituto Lillo, or in private libraries.

The author points out in his valuable introduction that content analysis reveals that some political newspapers failed for lack of funds or because of persecution by the local authorities, while others furthered the political ambitions of their founders and editors, many of whom became national senators and congressmen. Others reflected the struggle between and within the leading provincial families. A popular press does not appear until after 1890. However, even at that late date, provincial administrations were establishing their own journals and selecting editors from the provincial or municipal bureaucracy.

In sum, García Soriano has compiled the definitive list of pre-1900 newspapers in Tucumán. We need a similar work for each of the Argentine provinces.

JOSEPH T. CRISCENTI
Boston College

PETER H. SMITH. *Argentina and the Failure of Democracy: Conflict among Political Elites, 1904-1955*. Madison: University of Wisconsin Press. 1974. Pp. xx, 215. \$12.50.

Peter H. Smith's book is a thought-provoking examination of the failure of democratic institutions to take root in Argentina. He argues effectively

that the "pathological approach to Argentine politics, which has dominated so much of the literature . . . , is mis-placed and ill-conceived" (p. 111). That Argentina should have turned to authoritarian government "need not come as a surprise or a betrayal" (p. 112). Democratic institutions failed because of recurrent crises, logical though inadequate responses, and ultimate systemic collapse. Profound legitimacy crises in 1930, 1943, and 1955 helped trigger military intervention. Also important to the failure of democracy is the "dialectical relationship between the patterns of socioeconomic development and the patterns of political transition" (p. 113). When the socioeconomic elite began to lose control over the political order between 1916 and 1930 some sectors sought a restoration of government by *acuerdo*.

Such conclusions are highly speculative and demand further study and refinement. Virtually all of Smith's hypotheses rest on a sophisticated statistical analysis of the roll call votes in the Argentine Chamber of Deputies between 1904 and 1955. The substantive results are fascinating and richly suggestive. The methodology used, while impressive, should give pause for reflection, particularly the use of factor analysis, which has come under increasing attack by social scientists. Ambiguity in the categorization of factors is the most serious shortcoming of this complex methodology. Smith is well aware of the difficulties, and most of his findings are properly qualified.

With the aid of the computer Smith is able to survey Argentina's political transition from the closed system of the oligarchy, through the more open politics of middle-class Radicals, to the authoritarian populism of Perón as a "coherent and unified process" (p. xvii). The Chamber of Deputies, an unparalleled "arena of conflict and contention," is his explicative vehicle. Not all scholars will agree with Smith's assessment of the importance of the Chamber of Deputies as a mirror of society, or facets of society. Nor does voting analysis, as Smith realizes, necessarily reveal motivation. And what was the nature of debate and cleavage in committee—behind closed doors and beyond public scrutiny? Complaints aside, Smith's book is a significant, challenging, and important study that should serve as a springboard to further research.

PAUL B. GOODWIN, JR.
University of Connecticut

ALBERTO CIRIA. *Parties and Power in Modern Argentina (1930-1946)*. Translated by CARLOS A. ASTIZ, with MARY F. MCCARTHY. Albany: State University of New York Press. 1974. Pp. xv, 357. \$15.00.

More and more scholars interested in modern Argentina are focusing their attention on the critical,

and often neglected, period from 1930 to 1946. Among the most important additions to the growing literature in English on these years is this translation of a work by one of Argentina's most knowledgeable and perceptive writers. When Alberto Ciria's study first appeared in Spanish in 1964 it was widely acclaimed for its thorough documentation, its detailed analysis of a complex and confusing period, and its stimulating hypotheses. The second Spanish edition (1968) updated bibliographical references and expanded the final chapter to apply the patterns of the pre-Perón period to the post-Perón era. The current English edition is based on the second Spanish edition.

The book consists of two parts. The first presents a chronological description of Argentine political history from 1930 to 1946 organized around the four presidential administrations of Uriburu, Justo, Ortiz-Castillo, and Perón. The second part is an examination of important power groups: the political parties, the Church, the armed forces, the economic groups, and the labor movement. The analysis in both parts is supported by extensive reference to primary and secondary sources, in particular to the frequently overlooked *Diario de sesiones* of the Argentine Congress, and by nearly two dozen tables.

The importance of the book lies essentially in its explanation of the parliamentary crisis that has afflicted Argentina since the 1930s. Ciria's thesis is that the crisis is the result of the basic inapplicability to twentieth-century Argentina of the liberal political system established by the Constitution of 1853. Under external as well as internal pressure, the liberal system disintegrated during the 1930s. Political parties ceased to be the centers of political power and were replaced by the armed forces, by various economic groups, and, to a lesser extent, by the labor movement.

One may disagree with this thesis or with some of the subtheses of the author, but this is an important book on a critical period in Argentine history that is now available to the English-reading audience.

SAMUEL L. BAILY
Rutgers University

HAROLD BLAKEMORE. *British Nitrates and Chilean Politics, 1886-1896: Balmaceda and North*. (University of London Institute of Latin American Studies Monographs, number 4.) London: Athlone Press, and University of London, for the Institute; distrib. by Humanities Press, Atlantic Highlands, N.J. 1974. Pp. viii, 260. \$13.50.

Harold Blakemore demonstrates that Marxian interpreters, both national and foreign, have woven a web of mythology around the events of 1886-91 in Chile. The foremost hero to these interpreters has been José Manuel Balmaceda, the president of Chile during these troubled years that ended in

civil war. He has been depicted as an economic nationalist intent upon challenging the control over his country's economy wielded by British investors in league with an emerging Chilean bourgeoisie avid for personal profit and unconcerned with national well-being. The villain is John Thomas North who, born into reasonably favorable middle-class circumstances near Leeds in Yorkshire, became the most notable and visible foreign investor in Chilean nitrates and acquired the title of "the Nitrate King." Anxious to retain more nitrate income in Chile for use in his public works projects and other programs aimed at national betterment, Balmaceda is supposed to have clashed with North and his Chilean henchmen who in turn combined to precipitate the civil war that overthrew the president, setting the stage for his suicide in 1891. As a consequence of these events, Chile allegedly fell into the hands of international and national capitalists whose direction ushered in a prolonged period of exploitation resulting in a consistent economic drain that exacerbated and perpetuated underdevelopment.

Basing his study on a thorough use of published accounts and manuscript sources, Blakemore challenges this interpretation and effectively undermines its credibility, but he fails, in my judgment, to demolish it altogether. It would appear that there simply is not enough documentation available to resolve some of the points of the controversy, and the author is frank in conceding this.

In the brief concluding section of his valuable and dispassionate study Blakemore argues persuasively that Balmaceda was not antagonistic to foreign capital in itself. His ire was aroused only when foreign capital did not provide his regime desired amounts of income. Balmaceda was not seeking to end Chilean dependence on foreign capital but rather to increase that dependence under conditions permitting his government to derive a larger share of benefits to be used for immediate political advantage as much as for long-range national development. In this interpretation, Balmaceda emerges as a precursor of ever so many twentieth-century Latin American leaders whose economic nationalism must be viewed with skepticism. Balmaceda's relations with foreign capital, regardless of what motives shaped them, were not nearly so important in causing his downfall as purely internal political issues. Throughout his book Blakemore is effective in arguing this last point.

FREDRICK B. PIKE
University of Notre Dame

THEODORE H. MORAN. *Multinational Corporations and the Politics of Dependence: Copper in Chile*. (Written under the auspices of the Center for International Affairs, Harvard University.) Princeton: Princeton University Press. 1975. Pp. xiv, 286. \$12.50.

Theodore Moran's analysis of the relationship between multinational corporations, particularly Anaconda and Kennecott, and the Chilean economy and political system is a case study in the best sense of that term. It provides a test of prevailing dependency theory, while at the same time serving to generate a suggestive new model of the relationship between multinational corporations and their host countries.

By means of an analytical history of the formation of copper policy in Chile from the end of World War II through the end of the Allende regime, Moran shows that a bargaining, or balance of power, model better fits the Chilean experience than does the standard dependency model. The essence of the argument is that the multinational corporations had the bargaining advantage at the outset when the host country needed the capital and technology that only the businesses could provide. However, once large capital outlays had been committed, Chile tended to gain in technical and negotiating skills, gradually acquiring the upper hand until nationalization became almost inevitable. Concomitantly, the original coalition between foreign corporations and domestic elites shifted over time as well, until by the late 1960s Chilean industrial and agrarian elites themselves backed nationalization. Moran ends this book by recommending to Chile that its nationalized copper industry would best maximize its own economic interests by, in effect, joining the international copper oligopoly and playing by its rules.

There is little in this book concerning Chilean political history in the usual sense—Frei's victory in 1964 over Allende is even mistakenly called "narrow" (p. 127)—and political scientists and economists will probably welcome the book more than will historians. It is curious that there is only brief mention of those important Chilean political actors, the copper workers' unions. This is, however, a very impressive and innovative study, one that, while not rejecting dependency theory in its entirety, nonetheless brings the discussion of that theory down from the plane of rhetoric and ideology to the more fruitful level of empirically based analysis.

ROBERT DIX
Rice University

Recent Deaths

When on February 26, 1976, ARTHUR C. COLE died, the historical profession lost one of its most distinguished members. Born on April 22, 1886, at Ann Arbor, Cole was graduated from the University of Michigan, received his doctorate from the University of Pennsylvania, and in 1912 won the AHA's Justin Winsor Prize for *The Whig Party in the South*. From 1912 to 1920, he taught at the University of Illinois. In 1920 he became a professor of history at Ohio State and in 1930 moved to Western Reserve. From 1944 until 1956, he was a member of the Department of History at Brooklyn College and served as its chairman after 1950.

Cole was a leading scholar in his specialty. Managing Editor of the *Mississippi Valley Historical Review* from 1930 to 1940 and president of the Mississippi Valley Historical Association in 1940-41, he was one of the few authors of his era whose books on the sectional conflict have not been completely revised by modern scholars. Unlike most of his contemporaries, he never abandoned his belief in the justice of the antislavery cause. In *The Irrepressible Conflict* (1934) he forcefully argued the emergence of two civilizations as a cause of the Civil War.

Cole's contributions to scholarship were widely recognized. In addition to *The Irrepressible Conflict* and *The Whig Party in the South*—the latter a brilliant exposition of the coincidence of Whig voting strength and large-scale slave holding—he wrote *The Era of the Civil War* (the third volume of the *Centennial History of Illinois*) and *A Hundred Years of Mount Holyoke College*, and edited the *Constitutional Debates of 1847* in the *Collections* of the Illinois State Library. His exchange with Joseph G. de Roulhac Hamilton in the 1931 and 1932 *AHR* on "Lincoln's Election an Immediate Menace to Slavery in the States?" became famous (he took the negative view), and his many articles in learned and popular periodicals made him one of the best-known figures in his field.

Cole's commitment to libertarianism was not confined to his writing and teaching. Opposed to American involvement in World War I, he was

investigated by a committee of trustees at the University of Illinois. Subsequently, he played a prominent role in launching a teachers' union at the university. While at Ohio State, he challenged the program of compulsory military training, and as editor of the *Mississippi Valley Historical Review*, reviewed fairly W. E. B. DuBois' *Black Reconstruction*, which at that time was not even considered by other publications. He was an active member of the AAUP and in 1950 became chairman of the ACLU's Committee on Academic Freedom. During World War II, he served on a special Air Force committee to study the feasibility of reducing Germany by strategic bombing alone, a proposition which he correctly held to be erroneous.

Above all, Cole was a great human being. His interventions on behalf of colleagues and his skill as a teacher endeared him to faculty and students alike. The *Festschrift* intended for his ninetieth birthday will now have to be issued *in memoriam*, but it is certain that whenever American history is taught, the name of Arthur C. Cole will not be forgotten.

HANS L. TREFOUSSE
*Brooklyn College and Graduate Center,
City University of New York*

DANIEL COSÍO VILLEGAS, dean of historians of modern Mexico, died suddenly in his nation's capital on March 10, 1976, at the age of 77. He was a man of indefatigable energy, active until the end. In addition to his prolific historical writing, begun in middle life, Cosío had an extraordinary public career as a builder of institutions. From 1957 to 1968 he served as Mexico's special representative on the United Nations Economic and Social Council. In 1960 he presided over the Council.

Born in Mexico City on July 23, 1898, Cosío Villegas emerged in the early twenties as a brilliant student with a deep commitment to the regeneration of a nation racked by violent revolution. He studied abroad from 1925 to 1928, receiving an M.A. in economics from Cornell University. In 1934 he established the journal *El Trimestre Eco-*

nómico and the major publishing house, Fondo de Cultura Económica. As a diplomat in Lisbon in 1937, he initiated the project of inviting to Mexico a select group of Spanish intellectuals, refugees of the Civil War. A year later he became a cofounder of La Casa de España en México, later El Colegio de México, which is now one of the major graduate and research institutions in Latin America. He also founded in 1951 the principal national journal in the field, *Historia Mexicana*.

In 1948 Cosío Villegas began work on his magnum opus and greatest enterprise, the 9000-page *Historia moderna de México* (1955–1972). The collaborative *Historia* was a product of his famous “seminar,” which also served as a training school for young historians. Besides directing the project, Cosío wrote five of the nine volumes, those devoted to international and domestic politics between 1867 and 1910. Because of its massive documentation and its objectivity in a politicized but unstudied field, the *Historia moderna* has become the point of departure for all serious studies of the period. Soon after completing the final volume of the *Historia moderna*, Cosío started and codirected a new Seminario de la Historia de la Revolución Mexicana, the fruits of which are soon to appear.

The *Historia moderna* emerged from Cosío’s concern that the country after 1940 was drifting away from the ideals of constitutional liberalism that had moved the mid-nineteenth-century reformers, the leaders of the Restored Republic (1867–1876), and the early revolutionaries after 1910. For Cosío history must help guide the nation in the search for priorities. In his histories as in his much-read essays he spoke as a liberal, always lamenting the withering of democratic institutions, the authoritarianism, and the pervasive servility in government that made his era reminiscent of that of Porfirio Díaz (1876–1911). His Friday column in the newspaper *Excelsior* became an institution, unique for its penetrating and fearless criticism of the political system.

Though a commanding and dominant figure among his colleagues, Daniel Cosío Villegas was always open to new ideas. He maintained close contact with younger scholars and treated foreign investigators with generosity and respect. In short, he did much to give new life to the professional study of modern and contemporary Mexico, both within the country and abroad.

CHARLES A. HALE
University of Iowa

For various reasons, personal or circumstantial, it is not given to every historian to incarnate the period he has made his own. LEWIS PERRY CURTIS, who died at his home in New Haven on April 8, 1976, so steeped himself in eighteenth-century Eng-

lish politics and society that, with little make-up, he could have played Lord Chesterfield redivivus. The man who knew him best has affectionately recalled the eighteenth-century atmosphere of his household wherein, not altogether paradoxically, Georgian pronouncements assayed twentieth-century vagaries. However deeply committed to a particular past one may be, it is too late to inhabit it exclusively: the present will inspire comparisons. Among the many mirrors of his favorite world in which Curtis was reflected, two closely set together, the epistolary and instructive, may be seized upon as revealing his character and interests.

It is not surprising that a man whose major scholarly book was the meticulously edited *Letters of Laurence Sterne* (Oxford, 1935, 1965) would indulge in similar expression. Since Sterne’s wit and wisdom frequently turned in directions other than instruction, however, Curtis in his own letters to his son followed the Chesterfieldian line—learned, allusive, magisterial, and witty (in the eighteenth-century sense)—heightened by the insights of Burke and Gibbon. The flavor as well as the substance of the eighteenth century permeate all his writing, *The Politics of Sterne* (1929), *Anglican Moods* (1966), and especially the more personal idiosyncratic *Chichester Towers* (1966). Whatever he wrote, whether of Sterne or Gibbon or Archdeacon Ball, he never reduced his subjects to a statistic. That he was a brilliant as well as dedicated teacher comes as no surprise. An unregenerate humanist, he sought always to portray the whole culture by fusing its several parts, never better illustrated than by his understanding of the pervasive importance of religion, even at its most formal, in contemporary life.

Born at Southport, Connecticut, November 30, 1900, he began his professional career in 1927, after graduating Yale B.A. (1923), Ph.D. (1926), as instructor in English Literature at his alma mater, where he was to remain throughout his academic life. In 1933 he moved to the Department of History, without in any sense shifting his intellectual allegiance, retiring in 1969 as Colgate Professor of History. He held ACLS and Guggenheim fellowships in 1931–32 and 1941–42 respectively. During his forty-two years of service he sought constantly, by precept and practice alike, to promote the teaching of history. For him as for David Hume, the historian was a man who had lived from the beginning, and if men would exemplify the highest values they must know from whence they had come.

CHARLES F. MULLETT
University of Missouri

DOROTHY BURNE GOEBEL, professor emeritus of history at Hunter College of the City University of

New York, died on March 13, 1976, in Huntington, Long Island after a long illness. Mrs. Goebel was born in Huntington on August 24, 1898. She received the A.B. in 1920 from Barnard College, where she was elected to Phi Beta Kappa, and the M.A. in 1922 and the Ph.D. in 1926 from Columbia University. She was an assistant and lecturer in the history department at Barnard from 1920 to 1926 when she joined the history department at Hunter as instructor. Subsequently she became professor and served as chairman of the department, 1942-48 and 1961-62. She retired as professor emeritus in 1963.

As chairman and as a member of many important committees at Hunter, Dorothy Goebel made significant contributions to academic, educational, and scholarly achievements at the college. Within the department, in addition to giving distinguished administrative leadership, she played a leading role in originating and directing the required course in American history, which with its emphasis upon the interrelationship of the United States and other countries of the Western community was long a unique offering at Hunter.

A specialist in American history, she wrote *William Henry Harrison* (1926) and *American Foreign Policy: the Documentary Record, 1776-1960* (1961). She contributed to the *Dictionary of American Biography* and the *Dictionary of American History*. Articles appeared in the *American Historical Review*, *Journal of Economic History*, and *Annals of the American Academy of Political and Social Science*.

A striking part of her scholarly record was her cooperation with her husband, the late Julius Goebel, Jr., a distinguished legal historian and professor on the Law Faculty of Columbia University. Shortly after their marriage in 1925 she assisted in the survey of colonial court records in the state of New York that became the basis for Julius Goebel's first major contribution to the history of law, *Some Legal and Political Aspects of the Manors of New York* (1928). In 1945 the Goebels wrote *Generals in the White House*, a "lively study" of the military and political careers of nine generals who became president. From 1963 Mrs. Goebel was associate editor of the *Law Practice of Alexander Hamilton: Documents and Commentary*, edited by Julius Goebel. The first two of four projected volumes appeared before his death in 1973. The full extent of this partnership has been well expressed in the memorial volume for Julius Goebel prepared by the *Columbia Law Review*, which describes the extraordinary extent to which Dorothy and Julius Goebel shared professional, social, and cultural interests—and "sparkling wit."

Mrs. Goebel was a member of the American Historical Association from 1933, serving on the Albert J. Beveridge Award Committee, 1947-53, as chairman, 1951-53. She received a Guggenheim

Fellowship in 1947 to study the free ports of the West Indies. The results of this research appeared in the *William and Mary Quarterly* (July 1963) under the title "The New England Trade and the French West Indies, 1763-1774: a Study of Trade Policies."

Perhaps a final word is permissible for one who was a student in Dorothy Goebel's first history class at Barnard College fifty-four years ago and her colleague in the history department of Hunter College for many years. We respect the memory of a brilliant historian and gifted scholar, but we also cherish with affectionate admiration the personal integrity, the sensitivity of spirit, the exquisite taste, and the practical good sense so evident in her private as well as professional life.

MADELEINE HOOKE RICE
Hunter College,
City University of New York

ALFRED H. KELLY, professor of history at Wayne State University, died in Detroit on February 14, 1976. Born in Pekin, Illinois, on June 23, 1907, he completed his undergraduate and graduate work at the University of Chicago. He came to Wayne State University in 1935 to begin his career as a professor in the field of U.S. constitutional history and to be an administrator, an active and important faculty member, and a guide to thousands of students who knew how deeply interested he was in their hopes and achievements.

For twenty-two years Alfred Kelly was chairman of the Department of History. In 1953 he was appointed Franklin Lecturer. He served on numerous university committees and councils. He taught at the University of Tübingen and was decorated by the government of Italy. In 1954 he assisted Thurgood Marshall and others in preparing the brief filed in behalf of the NAACP in the case of *Brown v. Board of Education of Topeka* that resulted in a Supreme Court decision that soon came to symbolize the Court's determination to destroy segregation. In 1961 he was codirector for research and drafting for the Michigan Constitutional Convention. He was later appointed by President Nixon to the Permanent Committee for the Oliver Wendell Holmes Devise, responsible for preparing a history of the Supreme Court.

For several years Alfred Kelly was parliamentarian of the American Historical Association and the Organization of American Historians. At the time of his death he was Acting Dean of the Graduate School and a member of the Bicentennial Commission of Detroit.

Kelly lectured widely in the United States and Canada. He was a prolific writer of reviews, articles, and books, all of them marked by his usual logic and patterned precision. Among his best-known works are *The American Constitution: Its Origins and Development* (with W. A. Harbison), now in

its fifth edition, *American Foreign Policy and American Democracy*, and *Foundations of Freedom*. One of his last and finest papers was published by the Library of Congress in 1974 in *Leadership and the American Revolution*.

Those who knew Alfred Kelly well will remember him as a man of tremendous energy, much knowledge, eloquence, compassion, courage, integrity, insight, and humor. He was a distinguished professor. He was also a success as a person, and that is perhaps the highest praise that any man may have.

GOLDWIN SMITH
Wayne State University

GEORGE SIDNEY ROBERTS KITSON CLARK, a major contributor to the study of nineteenth-century British history, was born at Leeds, June 14, 1900. His life was intimately associated with Trinity College, Cambridge, where he came up as an undergraduate in 1919, and was a Fellow from 1922. He was Reader in Constitutional History at Cambridge from 1954 until his retirement in 1967. He had appointments as visiting lecturer at the University of Pennsylvania in 1953-54 and the University of Melbourne in 1964. He served as Ford's Lecturer at Oxford in 1959-60, as Maurice Lecturer at King's College, London, in 1960, and as Birkbeck Lecturer at Cambridge in 1967.

After his early book, *Peel and the Conservative Party* (1929), Kitson Clark published almost nothing for many years. He was heavily engaged in college and university teaching and in administrative tasks at Trinity College, where he served as tutor from 1933 to 1945. His most interesting work appeared in the last third of his life when he was over fifty, a period for him of substantial production and also of rapid intellectual growth. He attracted wide attention in the early 1950s by three excellent articles dealing with different phases of the politics of the 1840s. Some of the themes of these papers were picked up and further developed in books that dealt with the mid-nineteenth century in more general terms, notably *The Making of Victorian England* (1962), based on his Ford lectures, and *An Expanding Society: Britain 1830-1900* (1967). His interest in the history of religion in Britain and in its relation to other matters was reflected in *The English Inheritance* (1950) and *The Kingdom of Free Men* (1957), as well as in the last book he published during his lifetime, *Churchmen and the Condition of England, 1832-1855* (1973). He continued also in this last and most fruitful period to publish at intervals articles and long book reviews—really essays for which the book at hand furnished a starting point—that were substantial contributions to the discussion of limited subjects.

In dealing with the general questions that most interested him, Kitson Clark was discerning in

summarizing the existing state of knowledge and imaginative in bringing out the implications of his topic. Though conservative in some of his intellectual habits, he was also open-minded and welcomed new ideas and new approaches. He came, after some initial resistance, to have an informed and judicious understanding of what could be learned from statistics and systematic analysis and, though he did not much follow these lines himself, was cordial to those who did.

Kitson Clark was concerned not only with his own projects but also with the general advance of knowledge of his field, and his friendships with other historians and his contributions to the furthering of their work constituted an important part of his career. He was a famous research supervisor, and many of his students at Cambridge have now become distinguished scholars. His efforts, however, reached a wider audience. His continued interest in the problems faced by younger men resulted in his publication of several small tracts for their benefit on research methods, the location of research facilities, and the art of lecturing. He gave generously of his time and encouragement not only to his own students but also to those who came from elsewhere: I have seen something of this in his great kindness to my graduate students when I have given them introductions to him. Younger scholars, both in England and America, sent him their manuscripts, sometimes of book length, and, although he must have been overtaxed, he went through them painstakingly, offering suggestions that, in some cases, resulted in major recastings. He lived in a suite of big rooms lined with books over the Great Gate at Trinity College and, in these quarters, was wonderfully hospitable.

Kitson Clark never received a chair—many at Cambridge felt strongly that he should have—but his accomplishments were recognized in other ways, by honorary degrees from four universities, by the volume of essays in his honor edited by R. Robson in 1967, by his election as a Foreign Hon. Member of the American Academy of Arts and Sciences in 1975, and by the respect and affection of scholars in his field all over the world. He died suddenly and peacefully in his rooms at Trinity the night of December 8, 1975, while lying in bed reading *The Woman in White*. He was active and in good form up to the end and, in his last weeks, was working on a piece about the medical profession and social reform in the nineteenth century. His annotated edition of G. M. Young's *Victorian England: Portrait of an Age* was completed before his death.

WILLIAM O. AYDELOTTE
University of Iowa

LOUIS MORTON, American historian and the Daniel Webster Professor at Dartmouth College, died

in Burlington, Vermont, on February 13, 1976. Pre-eminent in the field of military history, Morton was also active as a commentator on contemporary national-security affairs and the teaching of history. He was always willing to lend his tireless energies and formidable talents to the work of the AHA and other scholarly organizations. He served Dartmouth with unstinting dedication; he joined its faculty in 1960, was chairman of its history department for six years, and in 1971-72 was acting provost.

Born in New York in 1913, Louis Morton received his B.S. and M.A. degrees from New York University, his doctorate from Duke University in 1938. His dissertation, "Robert Carter of Nomini Hall: A Virginia Tobacco Planter of the Eighteenth Century," was revised and published in 1941. Still in print, it stands as a small classic in the literature of colonial social history. War interrupted Morton's career as a colonialist after he had taught at the College of the City of New York and served as a researcher at Colonial Williamsburg. He entered the Army in 1942 and saw duty in the Pacific theater.

Then Kent Roberts Greenfield recruited him as one of that remarkable group of young historians who staffed the Office of the Chief of Military History to work on the official history of the Army in World War II. Morton remained in that post from 1946 to 1959, charged with supervising the series on the War in the Pacific. Its first volume was his magisterial study, *The Fall of the Philippines* (1953). It immediately won him recognition as a major figure in the field of military history.

Louis Morton never made half-way commitments. For him, the Army project was a venture that could give new meaning to "official" history—to make it professionally written history of the highest quality, free of apologetics, exploiting a unique opportunity to gain access to archival records and major historical actors. The task of bringing modern military history into the mainstream of historical scholarship absorbed him. "Here indeed," he wrote in a 1962 article on the historian and the study of war (*MVHR*, vol. 48), "is a task to challenge the historian, an opportunity to use his knowledge and skill to enlarge our understanding of war so that ultimately, if we cannot abolish it entirely, we may learn how to limit and control it so that it does not destroy us altogether."

His crowning scholarly achievement was his book *Strategy and Command: The First Two Years* (1962), which appeared in the Army series and is acknowledged to be one of the definitive works in his field. It reflects well Morton's view that history done properly embodies "scrupulous objectivity, imaginative reconstruction of past events, orderly

presentation, a broad knowledge of society, and an understanding of and appreciation for the dynamic forces of change." On this same premise, moreover, he contended that the historian's expertise ought to be brought to bear on the consideration of public policy, and his faith in the instrumental value of our discipline also reinforced his dedication to undergraduate teaching.

Program chairman for the AHA annual meeting in 1967, consulting editor to numerous journals, member of the history advisory groups of both the Army and the Air Force, president of the New England Historical Association, he was a professional's professional. His colleagues in these organizations doubtless remember him for his powerful intellect, his forceful personality, and his graciousness. Those of us who were Lou's colleagues at Dartmouth also felt the effects of his other gifts of wisdom and generosity. He was a superb teacher and a highly successful chairman, always lavishing time and energies on support of his younger colleagues' work. He was gladly recognized at Dartmouth as *primus inter pares* by his fellow historians and other faculty.

Many of his activities at Dartmouth reached out to affect the historical profession. Notable in this respect was his development of a model course in military history—one of only a few in the country—that not only was offered to undergraduates generally, but also served in lieu of the conventional ROTC military history sequence. He played a key role in organizing at Dartmouth the Daniel Webster Papers project, which became a major scholarly undertaking under the direction of his close friend Charles Wiltse. Typical of many of Morton's efforts over the years was his quiet (indeed anonymous) effort to see that a senior colleague was honored recently with a *Festschrift* of high scholarly quality. He also led in broadening the range of fields taught in the Dartmouth department and required of majors, to include those outside of European and United States history, far beyond what was conventionally done elsewhere.

Although skeptical of the hectic expansion of history graduate programs in the 1960s (he declined appointments at two major universities), Morton was frequently consulted at Dartmouth by graduate students from other institutions, and his services as lecturer were always in demand.

All the while, he continued to write journal articles in military history; he contributed a brace of brilliant essays to *Command Decisions* (ed., Kent Robert Greenfield, 1960); and he wrote on the historiography of his field in the *AHR*, in *World Politics*, and in other reviews. He was also coauthor of *Schools for Strategy: Education and Research in National Security Affairs* (1965). A major undertaking of the last years of his life, when uncomplainingly he

often worked in great physical pain as a heart ailment grew worse, was the splendid series that he conceived and edited, *The Macmillan Wars of the United States*.

Always a person of quiet strength and enormous integrity, Louis Morton leaves a wide circle of

friends whose association with him and with his family was a privileged opportunity. He also leaves the historical profession much in his debt.

HARRY N. SCHEIBER
*University of California,
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Other Books Received

Books listed were received by the *AHR* between March 1 and July 1, 1976. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

- Actes et documents du Saint-Siège relatifs à la seconde guerre mondiale. Volume 9, Le Saint-Siège et les victimes de la guerre, janvier-décembre 1943.* (Secrétairerie d'État de Sa Sainteté.) Vatican City: Libreria Editrice Vaticana. 1975. Pp. xxviii, 686.
- AITKEN, HUGH G. J. *Syntony and Spark—The Origins of Radio.* (Science, Culture, and Society: A Wiley-Interscience Series.) New York: John Wiley and Sons. 1976. Pp. xvi, 347.
- AMUNDSEN, ROALD. *The South Pole: An Account of the Norwegian Antarctic Expedition in the "Fram," 1910-1912*, in two volumes. Translated from the Norwegian by A. G. CHATER. Reprint. New York: Barnes and Noble. 1976. Pp. xxxv, 392; x, 449.
- BAFCHLER, JEAN. *Revolution.* Translated by JOAN VICKERS. (Key Concepts in the Social Sciences.) New York: Harper and Row. 1976. Pp. xxiv, 208. \$15.50.
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- BENTINCK-SMITH, WILLIAM. *Building a Great Library: The Coolidge Years at Harvard.* Cambridge, Mass.: Harvard University Press. 1976. Pp. xiii, 218. \$17.50.
- BEST, GEOFFREY, and WHEATCROFT, ANDREW, editors. *War, Economy and the Military Mind.* Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 136. \$10.00.
- BLACK, CYRIL E., edited with an introduction by. *Comparative Modernization: A Reader.* New York: Free Press. 1976. Pp. vi, 441. \$12.95.
- BLACKLEY, ROBERT. *Modern Revolutions and Revolutionists: A Bibliography.* Santa Barbara, Calif.: Clio Books. 1976. Pp. xxvii, 257. \$15.75.
- BRERETON, J. M. *The Horse in War.* New York: Arco Publishing. 1976. Pp. 160. \$12.95.
- BRINTON, CRANE, et al. *A History of Civilization.* Volume 1, *Prehistory to 1715*; volume 2, *1715 to the Present*. 5th ed. Englewood Cliffs, N.J.: Prentice-Hall. 1976. Pp. x, 432, xxxii; xvi, 436-928. \$10.95 each.
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- CARTTER, ALLAN M. *Ph.D.'s and the Academic Labor Market.* (Carnegie Commission on Higher Education Sponsored Research Studies.) New York: McGraw-Hill Book Company. 1976. Pp. xviii, 260. \$12.50.
- CHAUSSINAND-NOGARET, GUY, editor. *Une histoire des élites, 1700-1848: Recueil de textes présentés et commentés.* (École des Hautes Études en Sciences Sociales, Le savoir historique, number 6.) Paris: Mouton. 1975. Pp. 376. 64 fr.
- CIPOLLA, CARLO M., editor. *The Fontana Economic History of Europe.* Volume 3, *The Industrial Revolution, 1700-1914.* New York: Barnes and Noble. 1976. Pp. 624. \$19.50.
- CIPOLLA, CARLO M., editor. *The Fontana Economic History of Europe.* Volume 4, *The Emergence of Industrial Societies, Part 1.* New York: Barnes and Noble. 1976. Pp. 368. \$19.50.
- CORDIER, ANDREW W., and HARRELSON, MAX, editors. *Public Papers of the Secretaries-General of the United Nations.* Volume 6, *U Thant, 1961-1964.* New York: Columbia University Press. 1976. Pp. xviii, 708. \$27.50.
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- DEMAUSE, LLOYD, editor. *The New Psychohistory.* New York: Psychohistory Press. 1975. Pp. 313. \$12.95.
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- ELTON, G. R., editor. *Renaissance and Reformation, 1500-1648.* (Ideas and Institutions in Western Civilization.) 3rd ed. New York: Macmillan Company. 1976. Pp. xii, 362.
- FÜLLER, HEINRICH, et al., editors. *Konferenzen und Verträge, Verträge-Plöetz: Ein Handbuch geschichtlich bedeutsamer Zusammenkünfte und Vereinbarungen.* Volume 3, 1963-1970. Würzburg: Ploetz. 1975. Pp. xxviii, 484.
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- HEN-TOV, JACOB. *Communism and Zionism in Palestine: The Comintern and the Political Unrest in the 1920's*. Cambridge, Mass.: Schenkman Publishing Company. 1974. Pp. viii, 184.
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- JACOBY, HENRY. *The Bureaucratization of the World*. Translated from the German by EVELINE L. KANES. Berkeley and Los Angeles: University of California Press. 1976. Pp. vii, 241. \$4.95.
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- MEAKIN, DAVID. *Literature, Man and Culture and Industrial Work Society*. New York: Holmes and Meier. 1976. Pp. viii, 215. \$10.00.
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- MORGAN, PATRICK M. *Theories and Approaches to International Politics. What are We to Think?* 2d. ed. Palo Alto, Calif.: Page-Ficklin. 1976. Pp. xiii, 306.
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- ROUX, SIMONE. *La maison dans l'histoire*. (L'aventure humaine.) Paris: Albin Michel. 1976. Pp. 299. 39 fr.
- SAVORY, R. M., editor. *Introduction to Islamic Civilization*. New York: Cambridge University Press. 1976. Pp. viii, 204. Cloth \$17.95, paper \$5.95.
- SHUTMAN, DAVID. *An Annotated Bibliography of Cryptography*. (Garland Reference Library of the Humanities, volume 37.) New York: Garland Publishing. 1976. Pp. xvi, 218. \$35.00.
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sion of Society. New York: Cambridge University Press. 1976. Pp. ix, 374. \$15.95.

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- BICKERMAN, ELIAS, and SMITH, MORTON. *The Ancient History of Western Civilization*. New York: Harper and Row. 1976. Pp. xii, 274. \$6.95.
- BOREN, HENRY C. *The Ancient World: An Historical Perspective*. Englewood Cliffs, N.J.: Prentice-Hall. 1976. Pp. xv, 383. \$12.95.
- BRANNAN, P. T., editor. *Classica et Iberica: A Festschrift in Honor of The Reverend Joseph M.-F. Marique, S. J.* Worcester, Mass.: Holy Cross Institute for Early Christian Iberian Studies. 1975. Pp. viii, 425.
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- GRANT, MICHAEL. *The Twelve Caesars*. New York: Charles Scribner's Sons. 1975. Pp. xi, 282. \$12.50.
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- JEFFERY, L. H. *Archaic Greece: The City-States, c. 700-500 B.C.* New York: St. Martin's Press. 1976. Pp. 272. \$16.95.
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- MACKENDRICK, PAUL. *The Mute Stones Speak: The Story of Archaeology in Italy*. (The Norton Library.) Reprint. New York: W. W. Norton and Company. 1976. Pp. xiii, 369. \$3.95.
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WEBER, MAX. *The Agrarian Sociology of Ancient Civilizations*. Translated by R. I. FRANK. (Foundations of History Library.) Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. 421. \$21.50.

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- BOSL, KARL, editor. *Gesellschaft, Kultur, Literatur: Rezeption und Originalität im Wachsen einer europäischen Literatur und Geistigkeit. Beiträge Luitpold Wallach gewidmet*. (Monographien zur Geschichte des Mittelalters, volume 11.) Stuttgart: Anton Hiersemann. 1975. Pp. ix, 309. DM 130.
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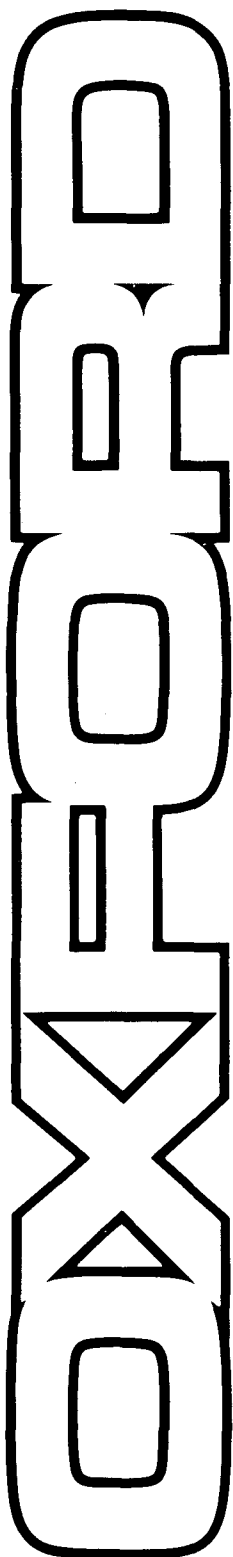
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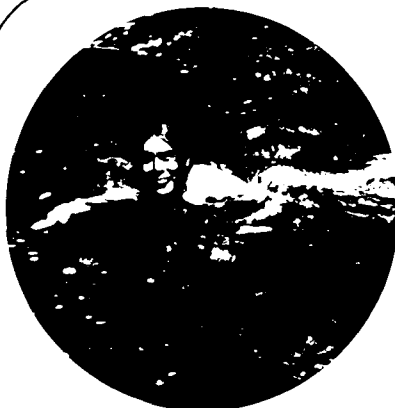
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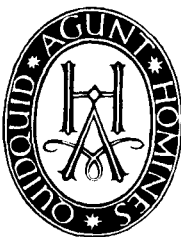
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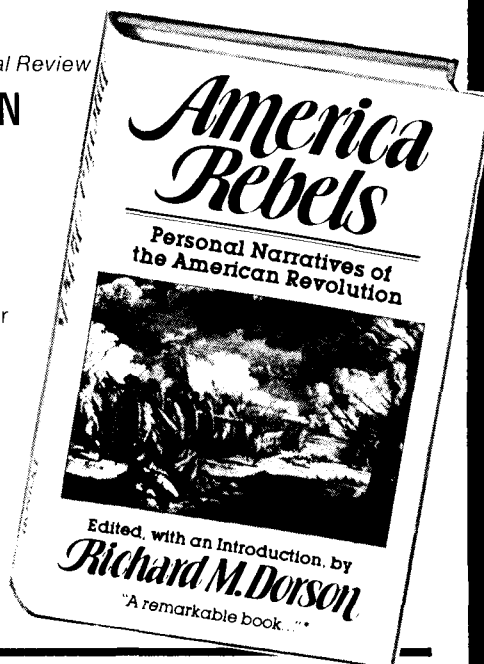
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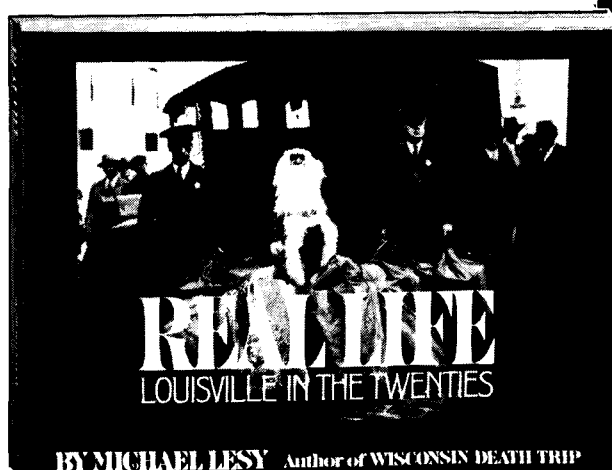
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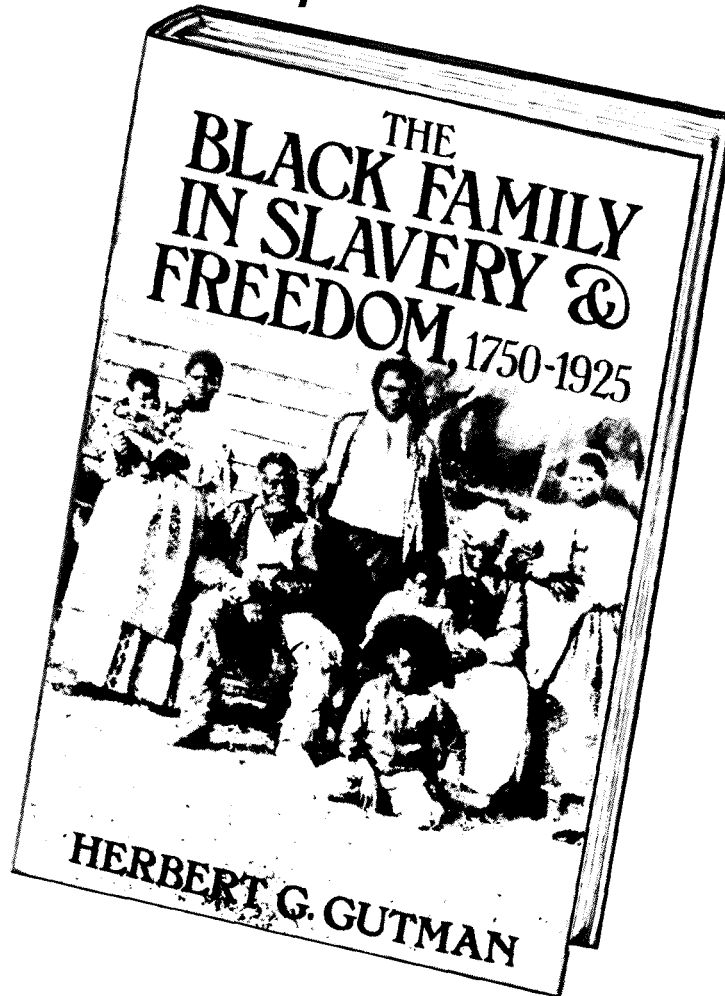
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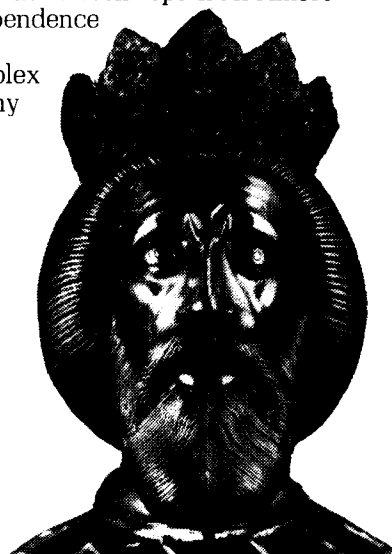
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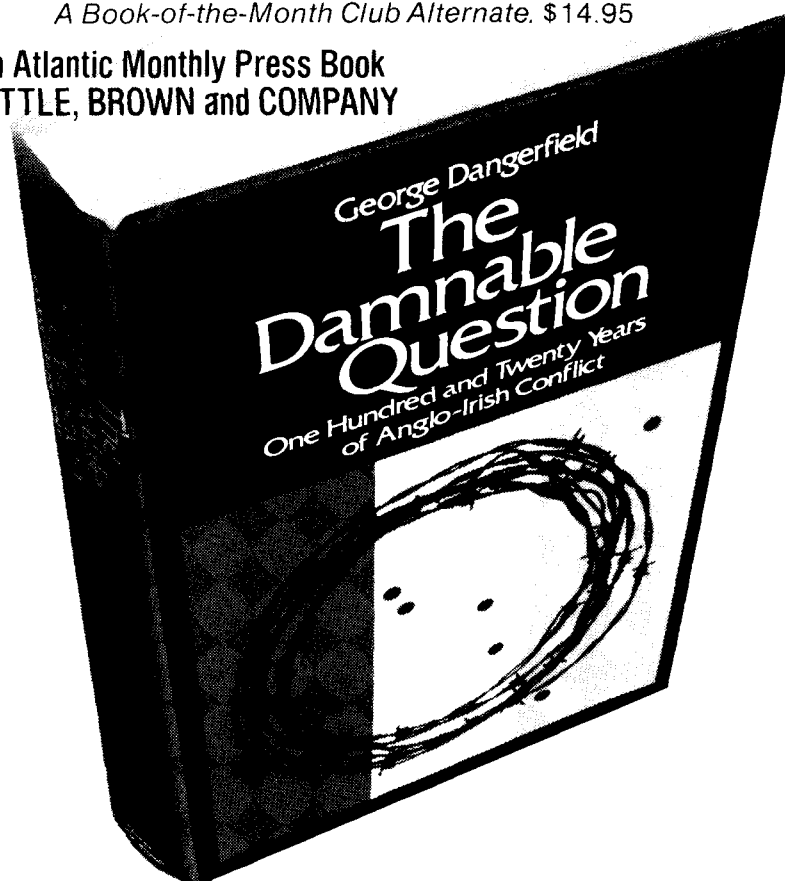
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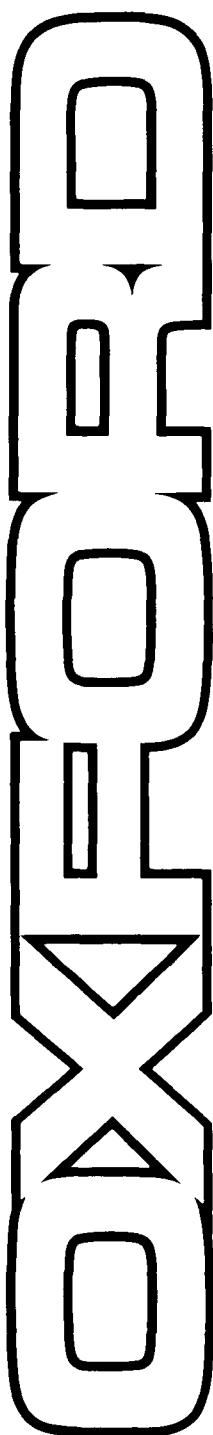
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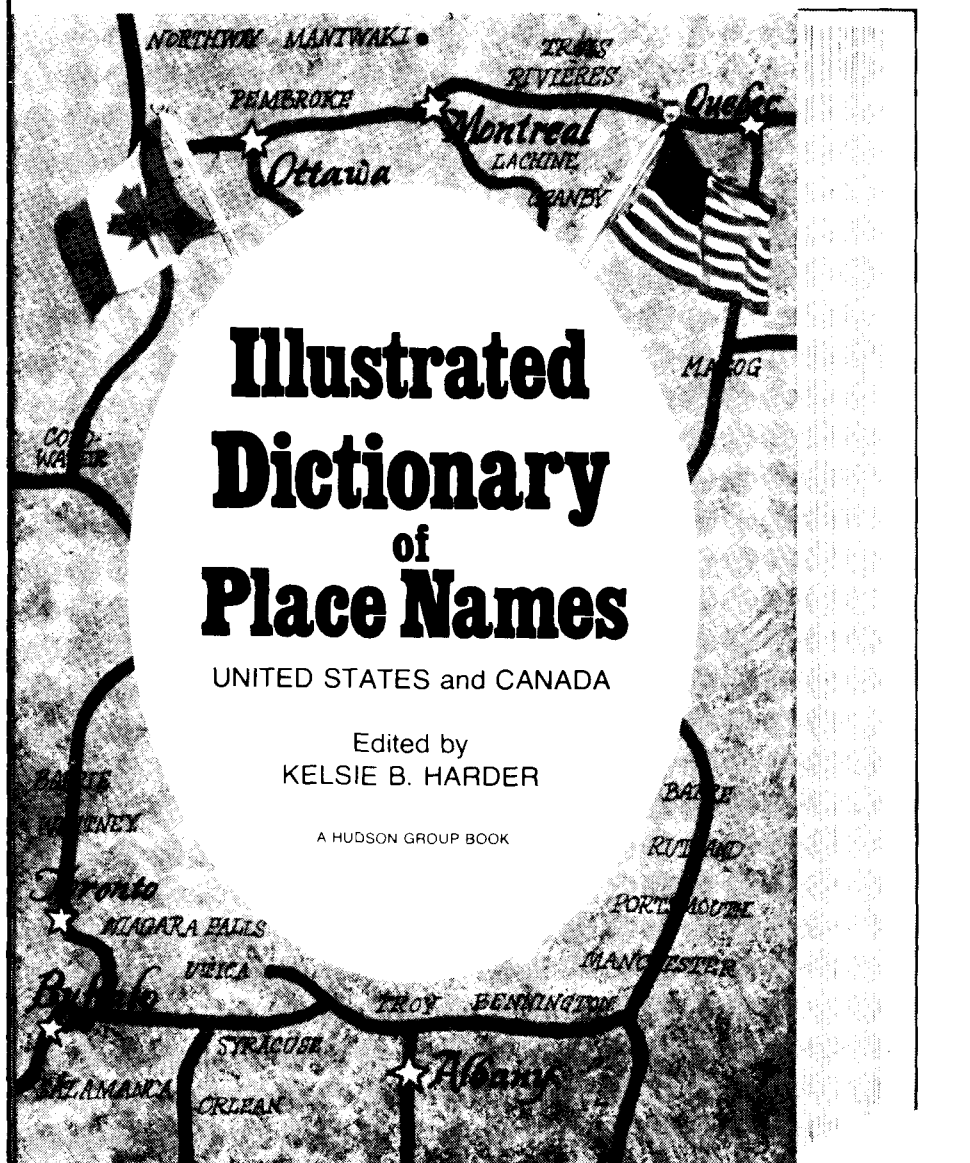
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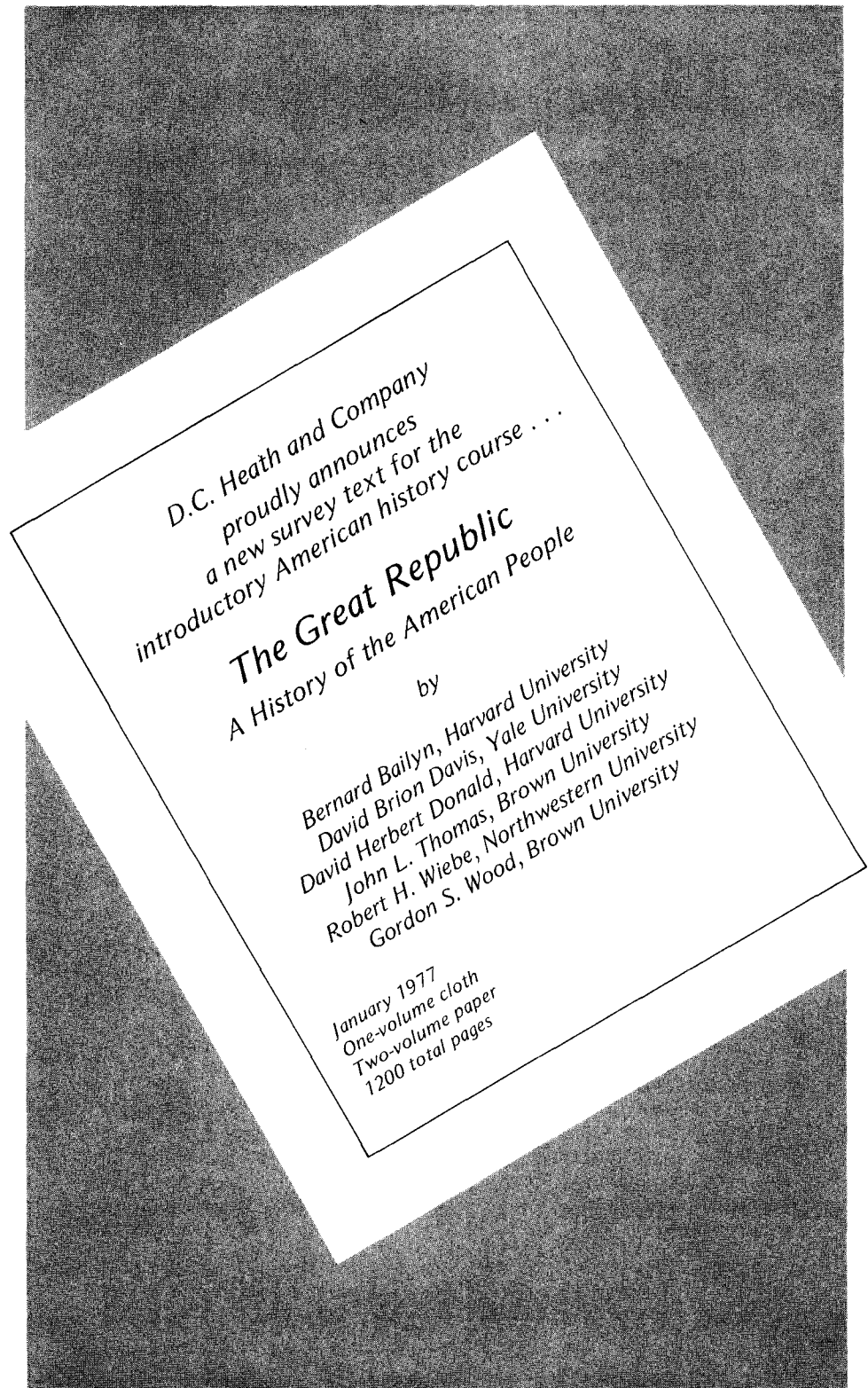
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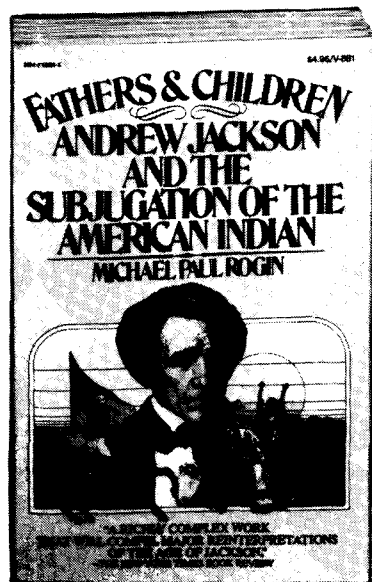
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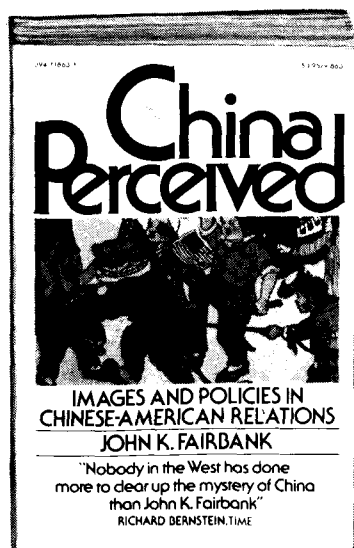
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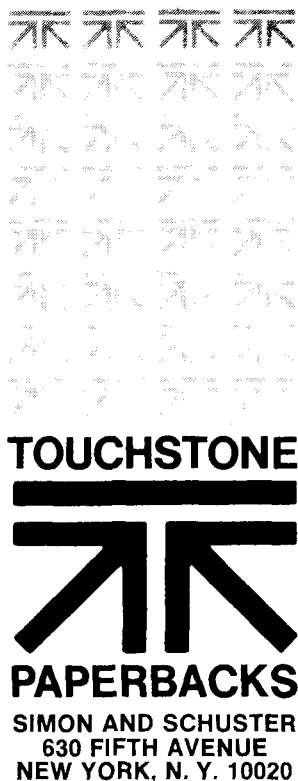
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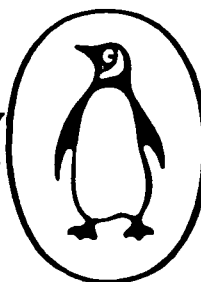
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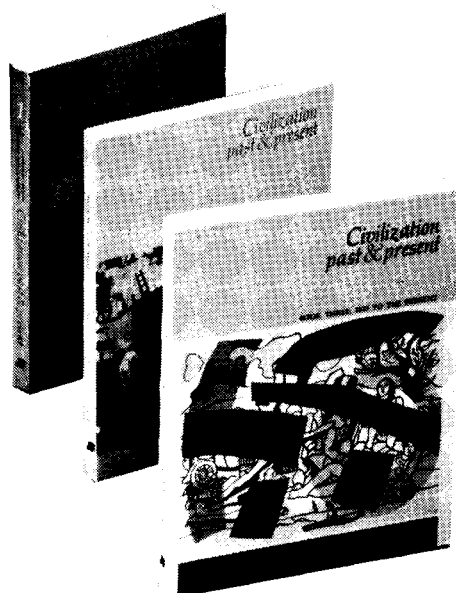
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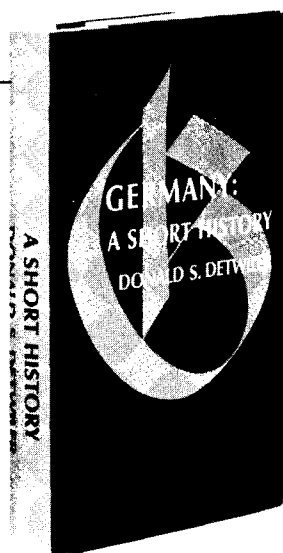
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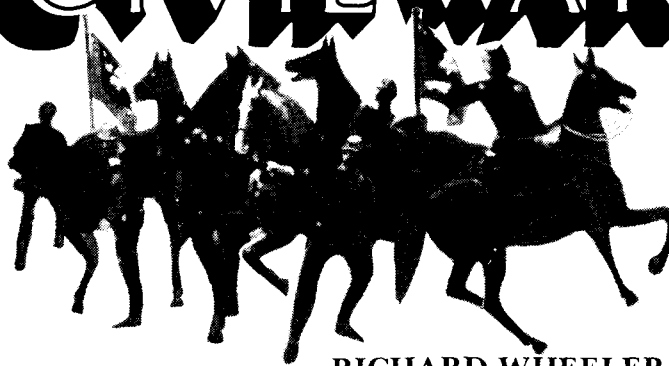
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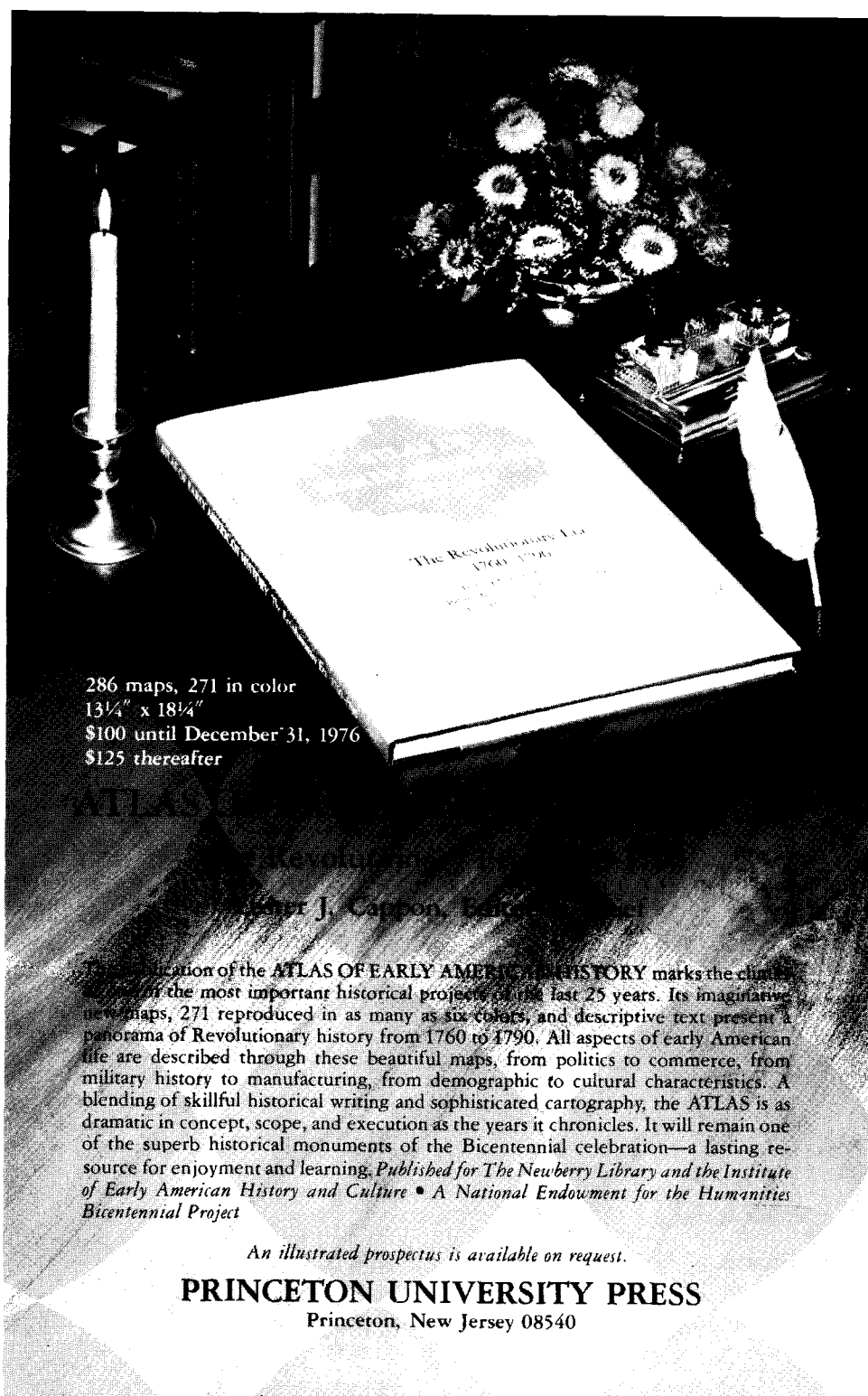
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